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ABSTRACTS

Political Regimes and the Extractive Capacity of Governments
Taxation in Democracies and Dictatorships

By JOSÉ ANTONIO CHEIBUB

Recent political and economic transformations in Latin America, Africa, Asia, and Eastern Europe have brought about a renewed interest in the incentives and capabilities of different types of political regimes to implement policies that are deemed necessary for economic development, in particular, policies aimed at increasing tax revenue. One central question is whether democracies can collect as much in taxes as dictatorships. This article addresses this question by examining whether regime type, classified as democracy or dictatorship, has a causal impact on a government's capacity to mobilize resources through taxation. On the basis of data gathered for 108 countries for the period between 1970 and 1990, the article concludes that observed differences across countries regarding the level of taxes collected by the government are not due to the fact that some are under a democracy and others under a dictatorship. Concerns about the inability of democratic regimes to collect taxes are, therefore, unfounded.

Why Do Formal Amendments Fail?
An Institutional Design Analysis

By CHRISTOPHER P. MANFREDI and MICHAEL LUSZTIG

Given the increasing importance of constitutional modification in Eastern Europe, Latin America, South Africa, Europe, and Australia, it is imperative to study systemically the conditions under which formal constitutional amendments are likely to fail. In this paper, the authors isolate conditions whereby the instrumental objectives of participants in the politics of constitutional modification threaten to overload the institutional capacity of the amending process. The proximate cause of amendment process overload is indeterminacy regarding the redistributive impact of the proposed change. Redistributive indeterminacy is a function of a rigid amending process, an institutional structure that encourages a large number of constitutional players and amendments which provide broad scope for judicial interpretation. The authors isolate two critical factors: the intensity of mass and elite preferences surrounding constitutional proposals and the structure of the amending process itself. They next trace these factors back to institutional variables to create a model to explain and predict constitutional failure in a range of contexts. Finally, they apply the model to highly visible recent constitutional failures in the United States and Canada.

Federalism and Central Bank Independence
The Politics of German Monetary Policy, 1957–92

By SUSANNE LOHMANN

Two channels of political control allow elected politicians to influence monetary policy. First, political threats to the status, structure, or very existence of the central bank may force central bankers to comply with politically motivated demands on monetary policy. Second, politicians may use their powers of appointment to ensure that central bank appointees share their electoral and party-political goals. This paper derives the monetary policy outcomes obtained as a function of the degree of central bank independence (zero, partial, or full) and central bankers' types (partisans or technocrats).

Based on a case study of the 1957 and 1992 institutional changes to the German central banking system and a regression analysis covering the period in between, the author argues that the *formal* independence of the system is protected by its embeddedness in the institutions of German federalism and by the federalist components of its decentralized organizational structure. The *behavioral* independence of the German central bank fluctuates over time with the party control of federalist veto points. The Bundesbank is staffed with nonpartisan technocrats who are partially insulated from political pressures.

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MOBILIZATION, SOCIAL NETWORKS, AND TURNOUT
EVIDENCE FROM JAPAN
By GARY W. COX, FRANCES M. ROSENBLUTH, and MICHAEL F. THIES

The strategic elites model of turnout argues that elites mobilize more when the probability of their effort deciding the electoral outcome is greater. Although the literature assumes that this probability depends solely on how close the election is, logically it depends jointly on how many votes are needed to affect the outcome (closeness) *and* on how many additional votes elite efforts are likely to garner (vote yield). Because the vote yield of mobilizational effort varies with the social capital of the district that elites face, the level of elite mobilizational effort (hence turnout) should depend *interactively* on closeness and social capital. The authors test their predictions using data from Japanese lower house elections for the years 1967–90. Japan is an interesting test case both because its (former) electoral system differs from that for which the model was first developed and because the literature clearly stresses the role of elite mobilization through social networks but does not examine the particular hypotheses advanced here.

DEMOCRACY AND DICTATORSHIP IN INTERWAR WESTERN EUROPE REVISITED
By THOMAS ERTMAN

Almost none of the conditions that, according to the latest research, favor democratic durability were present in Western Europe between the world wars. Yet only four Western European states became dictatorships during this period, whereas the others remained democratic despite economic crisis, an unhelpful international system, and the lure of nondemocratic alternatives. Several recent works offer new explanations for this pattern of interwar outcomes. Insofar as these works analyze the entire universe of Western European cases, they represent an important methodological advance. However, they remain too wedded to a class-coalitional framework to provide both a parsimonious and a historically accurate account of why democracy collapsed in some states but not in others. This article proposes an alternative explanatory framework that focuses on how political parties can shape associational life in such a way as to support or undermine democracy.

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POLITICAL REGIMES AND THE EXTRACTIVE CAPACITY OF GOVERNMENTS

Taxation in Democracies and Dictatorships

By JOSÉ ANTONIO CHEIBUB*

I. INTRODUCTION

THE simultaneous political and economic transformations occurring in many countries in Eastern Europe, Africa, Asia, and Latin America have brought about a renewed interest in the incentives and capabilities of different types of political regimes to implement policies deemed necessary for economic development. Those transformations raise questions about the conditions for successful economic performance in new democracies, as well as the prospects for their survival. On the one hand, democratization implies the emergence of governments that are dependent on popular support; they have to face the test of elections. On the other hand, economic crisis suggests the necessity of implementing policies that are likely to be unpopular, as they tend to reduce the level of present consumption, transform the structure of the economy, and, in the process, alter the current distribution of resources and harm entrenched interests. In the view of these considerations, we may ask, Will democracies be able to implement such policies? What will be the effect of these policies on the survival of democratic regimes?

In spite of the political and theoretical centrality of these questions, we know very little about how regimes interact with policy performance. Theoretical analyses have produced conflicting but plausible hypotheses about the effects of regimes on performance. Empirical analyses of the impact of regimes on performance, in turn, tend to be

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methodologically flawed since they rarely take into consideration, among other things, the effect of performance on regimes.[1]

In view of the inconclusive results produced by empirical analyses, several authors have recently suggested that the category "political regime" is too broad and should be disaggregated for analytical purposes.[2] Although it is undoubtedly important to consider political factors other than regime type to explain variations in policy performance across countries, it is too early to discard questions about the impact of political regimes from our research agenda. After all, the effect that political regimes may have on economic performance still challenges us empirically, theoretically, and politically.

This article assesses whether political regimes matter for a particular aspect of economic performance. Specifically, it addresses the question of whether regime type, classified as democracy or dictatorship, has a *causal* impact on the extractive capacity of a government, as measured by the level of taxes it collects. The fiscal crisis experienced by several countries in the recent past has led many analysts to doubt the capacity of those countries to raise revenue through taxation. For many observers, the resumption of economic development requires an increase in levels of public savings, which, in turn, requires an increase in taxation.[3] Yet, the capacity of governments to raise taxes, particularly under democratic conditions, is normally considered to be limited. One important task, therefore, is to assess whether differences in the level of

[1] Adam Przeworski and Fernando Limongi, "Political Regimes and Economic Growth," *Journal of Economic Perspectives* 7 (Summer 1993).

[2] See Alberto Alesina and Roberto Perotti, "The Political Economy of Growth: A Critical Survey of the Recent Literature," *World Bank Economic Review* 8 (September 1994); Barbara Geddes, "The Politics of Economic Liberalization," *Latin American Research Review* 30, no. 2 (1995); Stephan Haggard, *Pathways from the Periphery: The Politics of Growth in the Newly Industrialized Countries* (Ithaca, N. Y.: Cornell University Press, 1990); Stephan Haggard and Robert Kaufman, "Institutions and Economic Adjustment," in Haggard and Kaufman, eds., *The Politics of Economic Adjustment* (Princeton: Princeton University Press, 1992); Ben Ross Schneider, "Democratic Consolidation: Some Broad Comparisons and Sweeping Arguments," *Latin American Research Review* 30, no. 2 (1995).

[3] See, for instance, Luiz Carlos Bresser Pereira "Economic Reforms and Economic Growth: Efficiency and Politics in Latin America," in Luiz Carlos Bresser Pereira, José Maria Maravall, and Adam Przeworski, *Economic Reforms in New Democracies: A Social-Democratic Approach* (New York: Cambridge University Press, 1993); and Albert Fishlow, "The Latin American State," *Journal of Economic Perspectives* 4 (Summer 1990). John Williamson reports that revenue concerns were present in the tax reform proposals in the ten Latin American countries he examined. Richard M. Bird stresses the concern with increasing revenue in recent tax reforms in Latin America. The World Bank has recently recognized the importance of increased tax revenue in stabilization and reform programs for developing countries. Wiliamson, ed., *Latin American Adjustment: How Much Happened?* (Washington, D.C.: Institute of International Economics, 1990); Bird, "Tax Reform in Latin America: A Review of Some Recent Experiences," *Latin American Research Review* 27, no. 1 (1992); World Bank, *World Development Report 1988* (Washington, D.C.: World Bank, 1988); idem, *Lessons of Tax Reform* (Washington, D.C.: World Bank, 1991).

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taxes collected by each regime can be attributed to some inherent features of the regime. This is the main problem this article examines.

Section II discusses the arguments that postulate a relationship between the type of political regime and the level of taxes collected by the government, showing that there are competing hypotheses about this relationship. For that reason, we cannot be certain ex ante about the impact a political regime will have on the level of taxation. Section III develops an empirical model of the determinants of taxation, building on a number of propositions made in the public-finance literature. It specifies the factors for which we need to control in order to assess the impact of political regimes on taxation. Section IV presents the empirical analysis of taxation under democracies and dictatorships. The analysis is based on data for 108 countries roughly between 1970 and 1990. Section V summarizes the main findings and emphasizes the need to specify the ways in which political regime may or may not matter for economic performance.

II. Political Regimes and Taxation

A political regime is the broad institutional framework within which decisions concerning the production and allocation of public resources are made. It may affect economic performance directly through the environment it sustains, or indirectly via its influence on what governments are willing and able to do.

Existing theoretical essays offer contradictory arguments about taxation under different types of political regimes. Models developed by Przeworski, Findlay, and Olson, for example, suggest that authoritarian regimes have an incentive to tax at higher rates than democracies.[4] In those models, political regimes are distinguished by the rules regarding who decides about the size of the government and who appropriates the fiscal residuum. In one type of dictatorship, autocracies, state officials decide about the size of the government and privately appropriate the fiscal residuum. In another type, bureaucracies, state officials also make decisions about the size of the government but cannot privately appropriate the fiscal residuum. In the models, democracy serves as an ideal benchmark in the sense that governments are assumed to be perfect agents of citizens. Thus, in democratic regimes, citizens decide

[4] Adam Przeworski, *The State and the Economy under Capitalism* (Chur, Switzerland: Harwood Academic Publishers, 1990); Ronald Findlay, "The New Political Economy: Its Explanatory Power for LDCs," *Economics and Politics* 2 (July 1990); Mancur Olson, "Autocracy, Democracy, and Prosperity," in Richard J. Zeckhauser, ed., *Strategy and Choice* (Cambridge: MIT Press, 1991).

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through some kind of voting mechanism about the size of the government and have the right to appropriate the fiscal residuum. Given these distinctions, the models predict that the size of the government *as measured by the level of productive expenditures* will be close to the optimum in democracies, that is, close to the size that maximizes economic growth, too large in bureaucracies and too small in autocracies. As a consequence, economic growth will be higher in democracies than in dictatorships. Of more interest here, the models also suggest that the level of taxes will be higher in dictatorships than in democracies, even though it may differ in the two types of dictatorships. Although the models are based on strong assumptions and a somewhat naïve view of democracy,[5] they suggest that the level of taxes in democracies and dictatorships differs because of the incentives faced by governments under different regimes.

An alternative hypothesis suggests that democracies, not dictatorships, tax more and grow less than authoritarian regimes. Democracy, the argument goes, is a system in which public decisions must take into consideration the preferences of the majority of the population. For this reason, democratic governments are under constant pressure to meet the demands of labor unions and political parties for immediate consumption.[6] There are three reasons for this preference of labor unions and political parties. First, the rate at which the future is discounted correlates negatively with income level. Workers, who constitute the majority of the population, will be less willing than upper-income groups to postpone their consumption. Second, in the process of economic development, demands will increase much faster than the economy's capacity to satisfy them. The disparity will be experienced by the majority of the population as a deficit in consumption, thus providing an extra incentive for demanding increased current consumption. Finally, competition forces labor and party leaders to step up demands for immediate consumption, no matter how unwilling they are to do so.

The problem, according to this argument, is that pressures for immediate consumption, if satisfied, divert resources from investment, with negative consequences for growth. This may happen in three ways.[7] First, trade unions will successfully increase wages, which will imply reduced profit rates for capitalists, precisely the sector with a

[5] Mancur Olson is an exception since his results hold even if democracies are composed of groups with different levels of income, seeking redistribution in their favor. Olson (fn. 4).

[6] See Karl De Schweinitz, *Industrialization and Democracy* (New York: Free Press, 1964); and Walter Galenson, "Introduction," in Galenson, ed., *Labor and Economic Development* (New York: John Wiley, 1959).

[7] De Schweinitz (fn. 6).

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higher propensity to save. Second, workers will successfully obtain rules and regulations to protect labor, which may inhibit increments in efficiency due to experimentation with new forms of organizing production. Finally, political parties that receive support from labor will pressure the government to extend social benefits to workers. The consequences of the extension of social programs are twofold. On the one hand, due to their distributional consequences, these programs may cause the rate of investment to decrease. On the other hand, a higher level of expenditure must be financed by higher taxes, which will also have a negative impact on the capitalist's propensity to save. The argument comes full circle when one considers that, due to their dependence on the popular vote, democratic governments will have an incentive to respond to labor's demands for immediate consumption.[8] As a consequence, democracies are likely to grow less than dictatorships. And one of the mechanisms that will lead to this outcome is the relatively high level of taxes democratic governments will be forced to impose. Thus democratic competition will lead to increased taxation and, as a consequence, lower rates of economic growth.[9]

The arguments discussed so far refer to how political regimes affect a government's incentive to tax. There are also, however, hypotheses about the effect that political regimes have on a government's ability to collect taxes, regardless of the level of revenue it wants to raise.

Taxation represents the costs while public services represent the benefits of government. To the extent that government services approximate a public good, to pay taxes means to contribute to the provision of that public good. For this reason, as Bates says, "taxation inherently implies politics":[10] taxpayers have an incentive to misrepresent their

[8] This electoral dynamic does not require organized labor as a large sector of the population. It can be sustained in any system where labor constitutes a significant voting bloc or by any organization or political party that demands higher levels of social services and government expenditure.

[9] The idea that democracy undermines growth because it lowers the rate of investment is, of course, quite widespread. See Przeworski and Limongi (fn. 1) for a critical discussion of various hypotheses. I singled out this formulation because of the explicit role of taxation in the negative connection between democracy and growth. Note that De Schweinitz (fn. 6) does not consider the potential benefits of an increased amount of resources under the control of the state. His discussion implies that the fundamental role of the state in economic development is to affect the distribution of resources in order to place a disproportionate share of income in the hands of those who are considered to have a higher propensity to save. Taxation is detrimental to growth because it interferes with this process. However, this argument fails if, as Ragnar Nurkse has argued, private savings in developing countries are not forthcoming at the rate necessary to sustain reasonable levels of economic growth. Nurkse, "Growth in Under-Developed Countries: Some International Aspects of the Problem of Economic Development," *American Economic Review* 42 (May 1952).

[10] Robert H. Bates, "A Political Scientist Looks at Tax Reform," in Malcolm Gillis, ed., *Tax Reform in Developing Countries* (Durham, N.C.: Duke University Press, 1989), 479.

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preferences for public services in order to minimize their tax burdens and to shift costs to others. At the same time, due to the collective-action problem underlying taxation, governments face positive costs of tax collection. Compliance with tax laws cannot be assumed, even if these laws were passed to meet the demands of specific interest groups. From the perspective of individuals and firms—the actual taxpayers—the incentive to shift the burden of taxation, even a reduced burden, is permanent. To the extent that the use of coercion is also costly, some type of voluntary compliance is necessary if governments are to collect taxes.[11]

Arguments about the effect of political regimes on the government's ability to raise taxes focus on one of these consequences of the collective-action problem of taxation. On the one hand, those who believe that dictatorships are more able than democracies to collect taxes stress the former's capacity to use force in passing tax laws that yield larger revenues and in promoting compliance with these laws. On the other hand, those who see a democracy as being relatively more able to tax focus on its capacity to reduce the costs of compliance, which increases the level of revenue collected by the government.

The role of force is emphasized, for example, by Haggard, who writes that "collective action problems can be resolved by command" and suggests that dictatorships, supposedly, are more capable of issuing commands than democracies.[12] Authoritarian regimes, he argues, are not dependent on popular support, as are democracies. They are autonomous in the sense that they can disregard demands from voters and pressure groups on the design and implementation of policies. For this reason, they should be more able than democracies to extract resources from society. Haggard summarizes his argument in the following passage:

> Since authoritarian political arrangements give political elites autonomy from distributionist pressures, they increase the government's ability to extract resources, provide public goods, and impose the short-term costs associated with efficient economic adjustment. Weak legislatures that limit the representative role of parties, the corporatist organization of interest groups, and recourse to coercion in the face of resistance should all expand governments' freedom to maneuver on economic policy.[13]

Dictatorships, therefore, are free to impose any policy they choose. They base their power on force, which allows them to compel indi-

[11] Margaret Levi, *Of Rule and Revenue* (Berkeley: University of California Press, 1988).
[12] Haggard (fn. 2), 262.
[13] Ibid.

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viduals and groups to behave in ways that run counter to their self-interest.[14]

This argument does not make clear, however, exactly how the ability to use force is translated into a higher capacity to implement specific policies, particularly when those policies involve the behavior of decentralized, private agents. Force may affect tax policies in two ways. First, the government may repress the activities of interest groups who will try to obtain tax exemptions and favors, which, in turn, may have an effect on the level of revenue collected. Second, since authoritarian governments supposedly do not have to submit their policies to the electoral test, they may pass tax laws that might not be viable in a democratic regime. For instance, they may significantly increase statutory rates, broaden the tax net to include previously untaxed activities, or multiply the number of taxes they levy, even by taxing the same activity more than once under different guises.

But it does not necessarily follow in either case that the ability to use force will make authoritarian governments more capable than democracies of raising revenue. The image conveyed in the first case of an "unbound" dictator hardly corresponds to what we actually observe in most authoritarian regimes. Dictatorships are simply not as unconstrained as this image suggests. Even the harshest regime depends on some level of societal support to survive, which inevitably places a limit on its ability to extract resources from society.[15]

[14] The idea of an "unconstrained dictator" underlies several analyses of policy performance under different political regimes. For examples, see Oscar Oszlak, "Public Policies and Political Regimes in Latin America," *International Social Science Journal* 38, no. 2 (1986); and Benjamin A. Most, "Authoritarianisms and the Growth of the State in Latin America: An Assessment of Their Impact on Argentine Public Policy, 1930–1970," *Comparative Political Studies* 13 (July 1980). For critical discussions, see John W. Sloan and Kent T. Tedin, "The Consequences of Regime Type for Public Policy Outcomes," *Comparative Political Studies* 20 (April 1987); John W. Sloan, "The Policy Capabilities of Democratic Regimes in Latin America," *Latin American Research Review* 24, no. 2 (1989); and Karen L. Remmer, "Evaluating the Policy Impact of Military Regimes in Latin America," *Latin American Research Review* 13, no. 2 (1978).

[15] Barry Ames, *Political Survival: Politicians and Public Policy in Latin America* (Berkeley: University of California Press, 1987). One could argue that the issue is one of relative, not absolute, autonomy; that dictatorships are relatively more autonomous than democratic regimes and that, for this reason, they will be relatively more able to extract revenues from society. However, once the existence of such a limit is accepted, one needs to demonstrate that the degree of autonomy enjoyed by authoritarian governments enables them to pursue specific policies that democratic governments would not have been able to pursue. In other words, one has to demonstrate that the nature of the groups and coalitions that support authoritarian governments allows them a greater degree of freedom with respect to taxation (and other policies). But this can hardly be shown in general. As the history of many Latin American countries suggests, authoritarian governments that depend essentially on the support of only a few groups, say, large industrialists and landowners, are unlikely to be able to collect much revenue since these are precisely the biggest potential taxpayers. See, for example, M. H. Best, "Political Power and Tax Revenues in Central America," *Journal of Development Economics* 3 (March 1976); and Carmenza Gallo, *Taxes and State Power: Political Instability in Bolivia, 1900–1950* (Philadelphia: Temple University Press, 1991).

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A more important problem comes from the fact that there is no necessary correlation between the ability to use force, which is characteristic of dictatorships, and the development of the necessary apparatus for the effective collection of taxes. If it is true that authoritarian governments may have tax codes which, if fully implemented, would produce relatively higher levels of revenue, then it is also true that the level of revenue collected depends essentially on the existence of an effective system of tax administration. Such a system would be capable of monitoring the economy to identify potential tax bases, to assess tax liabilities in accordance with specific activities, and to collect effectively the revenue owed to the government.[16] Thus, even if dictatorships are able to pass harsher tax laws, they will not necessarily be able to translate these laws into actual revenues available for the government.

Finally, arguments about the compliance costs of government policies imply that democracies have a relatively higher capacity to collect taxes. For example, from Margaret Levi's discussion of "quasi-voluntary compliance" as a way to solve the collective-action problem of taxation, we can presume that democracies will have higher taxes than dictatorships.[17] According to Levi, quasi-voluntary compliance affects the level of taxes because it minimizes the compliance costs associated with taxation. For her, quasi-voluntary compliance depends on two conditions: the perception that there is a bargain between the government and citizens in which the government provides public goods that are positively valued in exchange for tax payment,[18] and the existence of assurances that the government and other taxpayers will keep their sides of the bargain. When these conditions hold, Levi argues, compliance with taxation may be expected to increase, reducing the costs of the enforcement of tax laws and increasing the revenue the government is able to collect.

Democratic regimes are more able than authoritarian ones to foster these conditions. In the first place, to the extent that elections produce some kind of a mandate, government policies may be interpreted as a contract between the government and voters in which the former is asked to perform certain tasks in exchange for compliance by the latter. Moreover, since elections are periodic, the government's performance will be subjected to popular scrutiny, thus giving it an incentive to keep its side of the bargain with taxpayers, or at least to promote the percep-

[16] Richard M. Bird, "The Administrative Dimensions of Tax Reform in Developing Countries," in Malcolm Gillis, ed., *Tax Reform in Developing Countries* (Durham, N.C.: Duke University Press, 1989).

[17] Levi (fn. 11).

[18] The importance of this factor is also emphasized by Bates (fn. 10).

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tion of doing so. The free flow of information characteristic of democratic regimes permits the transmission of the necessary information to the taxpayer to convey whether both the government and other taxpayers are keeping their sides of the bargain. Finally, as suggested by Levi's comparison of England and France, the Parliament is a forum where taxpayers, through their representatives, learn about the terms of the contract with the government, reveal their preferences for public goods, and engage in repeated interaction. All of these practices enhance the conditions leading to quasi-voluntary compliance. To the extent that they minimize the costs of enforcement associated with taxation, democracies will be relatively more able than dictatorships to collect revenue.[19]

Thus we can see that theoretical arguments do not provide a clear-cut answer to questions about levels of taxation under democracies and dictatorships. There are arguments about incentives and capabilities that suggest higher taxes in both. In the remainder of the paper I analyze this issue empirically. As we will see, the empirical analysis that follows suggests that there are no grounds for believing that democracies, particularly new democracies, are any less able than dictatorships to extract resources from society through taxation.

III. A Model of the Determinants of Taxation

Despite the existence of a vast literature on public finance, little is known about why countries have different systems and levels of taxation. The research in this area has failed to develop a coherent explanation that takes into consideration factors other than a few, clearly obvious structural characteristics that may affect a country's tax system. Political variables, for example, may reasonably have an impact on both the level and structure of taxation.[20] As a consequence, one does not find in the literature any adequate analytical model on which one may base a set of hypotheses about the nature of a country's tax system and the conditions under which existing systems may be successfully re-

[19] Another way to put it is that the legitimacy of democratic governments increases compliance with taxation, thus reducing enforcement costs and increasing revenue. According to Levi, quasi-voluntary compliance is one aspect of what is usually labeled legitimacy. In fact, as she argues, it provides microfoundations to the concept of legitimacy by separating analytically the elements that rest on ideology and norms and the elements that rest on strategic calculations and rational behavior. Levi (fn. 11), 54.

[20] Political variables are usually disregarded in both empirical and theoretical analyses of taxation. See Walter Hettich and Stanley Winer for a review of the models of government assumed in the main approaches to public finance. As they point out with respect to "optimal taxation," the most recent, sophisticated, and promising approach in the area of public finance, "political behavior is simply ignored." Hettich and Winer, "Blueprints and Pathways: The Shifting Foundation of Tax Reform," *National Tax Journal* 38 (December 1985), 440.

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formed. In view of this fact, this study is essentially inductive. It builds on a number of propositions about taxation scattered through the public-finance literature in order to build a model that may serve as a baseline for studying the effects of political regimes on levels of taxation.

Building on Levi's ideas there are four factors besides regime type that may affect the level of taxes the government collects: transaction costs, the rate at which the government discounts the future, the government's bargaining power with respect to society, and the government's overall fiscal situation. Together these factors make up a model of the determinants of taxation, which may be summarized as:

$$\text{Level of Taxation} = F(\text{Transaction Costs, Discount Rate, Bargaining Power, Fiscal Requirements}) \quad (1)$$

Transaction costs are the costs of implementing government policies. Ceteris paribus, governments will attempt to minimize the transaction costs associated with the policies they choose to implement. The most important transaction costs associated with taxation are due to difficulties in identifying and assessing the base on which revenue should be collected and in collecting the revenue itself. The existence of "tax handles" alleviates both difficulties since they represent bases that are easily defined and monitored, that is, they provide opportunities for revenue production at relatively low costs.[21]

In this study, transaction costs associated with taxation are captured by four variables, which are the most commonly used variables in studies that attempt to estimate the impact of tax handles on the level of taxes:[22] per capita GDP (LEVEL), the GDP share of imports and exports (TRADE), agricultural production (AGRIC), and the share of mineral exports in total exports (MINERAL). Per capita income indicates the availability of resources to be taxed, as well as the existence of administrative capabilities for collecting taxes: at higher levels of per capita income, economies tend to be more monetized and less informal, making

[21] Richard Musgrave, *Fiscal Systems* (New Haven: Yale University Press, 1969).

[22] See Roy W. Bahl, "A Regression Approach to Tax Effort and Tax Ratio Analysis," *IMF Staff Papers* 18 (November 1971); Raja J. Chelliah, "Trends in Taxation in Developing Countries," *IMF Staff Papers* 18 (July 1971); Raja J. Chelliah, Hessel J. Baas, and Margaret R. Kelly, "Tax Ratios and Tax Effort in Developing Countries, 1969–71," *IMF Staff Papers* 22 (March 1975); Jane H. Leuthold, "Tax Shares in Developing Economies: A Panel Study," *Journal of Development Economics* 35 (January 1991); Jorgen R. Lotz and Elliott R. Morss, "Measuring 'Tax Effort' in Developing Countries," *IMF Staff Papers* 14 (November 1967); and Alan A. Tait, Wilfrid L. M. Gratz, and Barry J. Eichengreen, "International Comparisons of Taxation for Selected Developing Countries, 1972–76," *IMF Staff Papers* 26 (March 1979).

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it easier for the government to collect taxes.[23] As a measure of the relative importance of foreign trade, the GDP share of imports and exports indicates the presence of a handle and should be positively associated with the level of taxation. From an administrative point of view, revenues collected at the border are among the least difficult to obtain. Because they have to pass through a few checkpoints to leave or enter a territory legally, imports and exports form a base upon which governments may impose a tax with relative ease. For this reason, foreign trade taxes have tended to represent the main source of government income during early phases of consolidation of the nation-state (in Europe during the seventeenth and eighteenth centuries, in Latin America and the United States in the nineteenth century, in Africa since the 1950s and 1960s.)[24] To the extent that it indicates the relative importance of a subsistence sector in the economy, the share of agriculture represents the absence of a handle and should, therefore, have a negative impact on taxation.[25]

The impact of mineral exports on the level of taxes is ambiguous. According to Raja J. Chelliah, a profitable, large, mineral sector, usually composed of large firms, may present the government with "a combination of taxable 'surplus' and administrative ease."[26] The level of taxes, therefore, should be higher when mineral production is relatively more important. The profitability of the mineral sector, however, depends on the prices that the mining products obtain in world markets, which, as we know from the history of so many developing countries, can fluctuate considerably over the years. Moreover, the positive effect of mineral production on the level of taxes is predicated on the fact that taxation is the main mechanism that governments use to appropriate resources generated in the mineral sector. This, however, is hardly the case since, in many countries, the most significant mineral production is directly controlled by the government, and state-owned firms do not always pay taxes.[27] Since the available data on central government revenue do not include the operation of state-owned enterprises, they exclude the non-

[23] Musgrave (fn. 21).

[24] See Daniel Tarschys, "Tributes, Tariffs, Taxes and Trade: The Changing Sources of Government Revenue," *British Journal of Political Science* 18 (January 1988); and Gabriel Ardant, "Financial Policy and Economic Infrastructure of Modern States and Nations," in Charles Tilly, ed., *The Formation of National States in Western Europe* (Princeton: Princeton University Press, 1975).

[25] See Lewis W. Snider, "The Political Performance of Third World Governments and the Debt Crisis," *American Political Science Review* 84 (December 1990).

[26] Chelliah (fn. 22), 295.

[27] In December 1992, Brazilian newspapers reported that Petrobras, the state oil monopoly, was the fisc's largest debtor.

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tax income that these sectors generate. Thus, even if the government derives considerable income from a relatively large mining sector, that income is not going to be reflected in the data on which this study is based.[28] Finally, to the extent that governments derive nontax income from the large mineral sectors they control directly, they may be in a position to avoid the politically sensitive task of taxing their population.[29] For those reasons, a relatively important mineral sector, directly controlled by the government, could imply lower, rather than higher levels of taxation.

The government's discount rate indicates the degree to which the government cares about what will happen in the future. If governments discount the future at high rates, they will be guided by short-term rather than long-term concerns. The implication of this fact for the level of taxation is ambiguous. Levi believes that governments with a high discount rate will plunder society by attempting to extract the maximum in taxes. If that is the case, taxation should increase with the rate at which governments discount the future. Cukierman, Edwards, and Tabellini hold a different view, partly due to a different specification of the government's objectives.[30] For them, in unstable and polarized societies, governments are likely to discount the future at high rates and have an incentive to rely on tax systems that facilitate tax evasion and impose high tax-collection costs. Those governments will prefer to finance their own activities with short-term instruments such as the inflation tax, and maintain a tax system with a low capacity of revenue production to constrain the behavior of future governments. If that scenario is true, then the level of taxes should decrease with the rate at which governments discount the future.

In this study, the government's discount rate is indicated by a variable, DISCOUNT, defined as the risk faced by chief executives of being removed from office, given the length of their tenure in office, the rate of economic growth, and the rate of past executive turnover in each

[28] Unfortunately, there are no comparative data on state-owned enterprises that cover as many countries as the ones included in this analysis. Such an exclusion need not bias the results reported here since a large public sector may either require capital outlays, thus exerting pressures for higher taxation, or provide revenue for the government, thus easing the pressure of taxation.

[29] For an example concerning Venezuela, see Arthur J. Mann, "On the Utility of Tax Performance Indices: Notes with Reference to Venezuela's Tax System," *Bulletin for International Fiscal Documentation* 31 (October 1977).

[30] Alex Cukierman, Sebastian Edwards, and Guido Tabellini, "Seigniorage and Political Instability," *American Economic Review* 82 (June 1992). See also Sebastian Edwards and Guido Tabellini, "Explaining Fiscal Policies and Inflation in Developing Countries," *Journal of International Money and Finance* 10 (March 1991); and idem, "Political Instability, Political Weakness and the Inflation Tax," NBER Working Paper, no. 3721 (Cambridge: National Bureau of Economic Research, 1991).

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country.[31] Governments are said to have high rates of discount when the risk of being removed from office is high. As just discussed, the effect of this variable on the level of taxation cannot be predicted ex ante. A high discount rate may lead the government to plunder society or may provide it with an incentive to rely on nontax sources of revenue, thus producing relatively low levels of taxation.

The government's relative bargaining power is, according to Levi, a function of the resources it controls. Its bargaining power increases when it does not depend on resources—economic, political, and otherwise—that are controlled by individuals and groups in society. It decreases when there are contenders for the allegiance of society's members.[32] Although it is difficult to measure relative bargaining power, we may obtain some idea of how it varies by considering situations in which governments tend to be generally more vulnerable to society or dependent on it. Elections, changes in regime, and changes in administration are, I believe, examples of such situations. In times of elections, voters control what a government wants and, given the existence of challengers, have the alternative to reward other power contenders with their votes. A government's bargaining power in these situations is likely to be relatively low.[33]

[31] This is the "hazard rate" produced by a model of the survival of chief executives in office as a function of length of tenure, economic growth, and the number of accumulated changes in chief executive positions experienced in a country since 1950, assuming a Weibull distribution ($p = 1.12$, with confidence interval from 1.02 to 1.21). Alternative specifications of the survival function do not change the results. Chief executives are presidents in presidential democracies, prime ministers in the parliamentary and mixed systems, and whoever is the effective ruler in dictatorships. The latter can be designated explicitly as the dictator, or can bear such titles as the head of the military junta, president, leader of the ruling party, executor of the state of emergency, or king. Chief executives were coded on the basis of the work of John V. DaGraça, *Heads of State and Government* (New York: New York University Press, 1985); Henry Bienen and Nicolas van de Walle, *Of Time and Power: Leadership Duration in the Modern World* (Stanford: Stanford University Press, 1991); and Arthur S. Banks, *Cross-National Time-Series Data Archive* (Center for Social Analysis, SUNY at Binghamton, 1993), magnetic tape. One limitation of this indicator of discount rate is that it is entirely backward-looking. An ideal operationalization of this factor would also consider the government's perceptions of its ability to alter its future chances of survival. I owe this observation to Jon Elster.

[32] Douglass C. North, *Structure and Change in Economic History* (New York: Norton, 1981), 28–29.

[33] This should be true of any type of elections, even the ones that occur under dictatorships. Authoritarian governments who subject themselves to popular elections do so in an attempt to legitimize their rule, to enlarge their base of support, and to consolidate their hold on power. They are, thus, also dependent on resources controlled by voters and, at least to some degree, are subject to the uncertainties inherent to electoral competition. Note that the frequency of elections is surprisingly high in dictatorships: information on 135 countries since 1950 indicates that presidential elections were held in 5 percent of the years under authoritarianism and 6 percent of the years under a democracy. On the one hand, legislative elections were more frequent under democracies than under dictatorships: 27 percent against 18.1 percent. On the other hand, 107 of the 231 presidential elections held since 1950 took place in democracies and 124 in dictatorships; of the 890 legislative elections during the same period, 457 were held in democracies and 433 in dictatorships. See José Antonio Cheibub and Adam Przeworski, "Democracy, Elections and Accountability for Economic Outcomes," in Bernard Manin, Susan Stokes, and Adam Przeworski, eds., *Democracy and Accountability* (New York: Cambridge University Press, forthcoming).

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Thus, elections, even the imperfect ones that take place in authoritarian regimes, affect a government's bargaining power negatively and, for this reason, can be expected to affect the level of taxes it is able to extract from society.

Parallel arguments can be made for situations in which there is a change in government or, more dramatically, a change in regime. Those are situations in which the government's normal functioning is disrupted and a new modus operandi needs to be established for decision making and policy implementation. In these cases, new governments are likely to be more vulnerable since they depend on other actors, notably the bureaucracy, for the implementation of their policies.[34]

Thus, the government's bargaining power was controlled by a series of dummy variables indicating situations in which governments may be expected to be more dependent on resources controlled by other actors in society: ELECTION (a variable coded 1 for the years in which a presidential or legislative election occurred), ADM (coded 1 for the years in which the chief executive changed), TRANS (coded 1 for the years in which a regime transition occurred). Since these variables indicate situations of low bargaining power by the government, the level of taxes should correlate negatively with them.

Finally, levels of taxation are also influenced by the government's overall fiscal situation. Tabellini points out that most empirical studies of the determinants of taxation assume that governments decide on the amount of taxes to collect independently of considerations of public expenditures and the existence of alternative sources of revenue.[35] The model of government underlying these studies assumes that fiscal authorities first collect whatever revenue they can and, on the basis of what they obtain, they then decide about the appropriate level of both public expenditures and nontax revenue. Since this is not the case, a better model of taxation should include variables indicating the expected level of activities to be financed by the government, as well as alternative sources of revenue. In this study these factors were indicated by two variables: the ratio of service on the outstanding public foreign debt to exports (DEBT) and the share of grants in the country's GDP

[34] Again, there is no reason for this to happen only in one type of regime. The dependence of new governments on external actors should be similar in both authoritarian and democratic regimes. Regime transition was more frequent in dictatorships than in democracies: of the eighty-nine transitions since 1950, forty-nine represented the death of a dictatorship. Changes in the executive, in turn, were more frequent in democracies: 60 percent of all changes since 1950 occurred in democracies. See Cheibub and Przeworski (fn. 33).

[35] Guido Tabellini, "International Tax Comparisons Reconsidered" (Manuscript, Fiscal Affairs Department, International Monetary Fund, 1985).

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(GRANTS).[36] Thus, the empirical form of the baseline model of taxation represented in equation 1 is:[37]

$$\text{TAX/GDP} = F(\text{Constant, LEVEL, TRADE, AGRIC, MINERAL, DISCOUNT, ELECTION, ADM, TRANS, DEBT, GRANTS}) + e \quad (2)$$

Table 1 presents the estimated coefficients for the variables in equation 2. The first column presents the OLS estimation of the baseline model of taxation, while the second column presents the fixed-effect estimation of the same model, corrected for first-order autocorrelation.[38] As we can see from this second column, all of the factors associated with transaction costs are relevant in accounting for variations in the level of taxation across countries. Taxes increase with per capita income and with the volume of foreign trade relative to the GDP; they decrease with the size of the agricultural sector and with the relative importance of mineral production. Taxes also increase significantly when governments discount the future heavily, supporting the view that governments will plunder when their tenure in office is at risk. Among the factors associated with the government's bargaining power, only the occurrence of elections matters for taxation: the level of taxes, as expected, is lower in electoral years than in nonelectoral years. Changes in the chief executive and changes in regime do not affect the level of taxes collected by the government. Finally, the effect of the govern-

[36] Ibid. In his study Tabellini also includes the noninterest level of public expenditures as an explanatory variable when estimating the level of taxation for sixty-one developing countries. This, however, introduces a serious simultaneity bias into the estimation (p. 17). The government's fiscal situation also depends on alternative sources of revenue such as nontax and seigniorage revenue. The inclusion of these variables in the analysis, however, may lead to multicollinearity since the same factors that affect the level of tax revenue probably also influence nontax and seigniorage revenue. To avoid this problem, one could follow Tabellini's recommendation and use total revenue, defined as the sum of tax, capital, and seigniorage revenue, as the dependent variable. Here, however, we want to know something about the government's willingness and ability to tax, as opposed to its willingness and ability to extract revenue from society, regardless of its form. This distinction is particularly relevant given that the increasing of tax revenue seems to be one of the paths out of the fiscal crises that have troubled many developing countries in the past decade. Moreover, tax revenue represents 83 percent of the total revenue in the countries for which data are available.

[37] This equation was estimated with information produced by the International Monetary Fund, the most comprehensive set of comparative data on taxation available for researchers. Data on levels of taxation are available for 108 countries from as early as 1970 through 1990, amounting to 1613 observations. Details about the coverage are presented in the appendix. World Bank, *World Data 1994: World Bank Indicators on CD-ROM* (Washington, D.C.: World Bank, 1994). A replication data set is available at the author's web site, at http://www.ssc.upenn.edu/polisci/faculty/Cheibub_Jose.html.

[38] ρ was computed as $1-\frac{1}{2}d$, where d is the Durbin-Watson statistic produced by the OLS estimation. The number of observations varies due to the availability of data on the independent variables and the loss of observations in estimating models with autocorrelated errors.

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TABLE 1
DETERMINANTS OF TAXATION (EQUATION 2)
(OLS AND COUNTRY-SPECIFIC FIXED-EFFECTS ESTIMATION, CORRECTED FOR AUTOCORRELATION)

Variable[a]	*OLS*	*Fixed Effects*	*Fixed Effects*
Constant	7.410		
	(5.99)[b]		
LEVEL	0.094	0.040	0.039
	(11.69)	(1.97)	(1.96)
TRADE	0.138	0.047	0.048
	(20.17)	(4.98)	(4.98)
AGRIC	−0.098	−0.172	−0.165
	(−4.36)	(−4.94)	(−4.73)
MINERAL	0.024	−0.044	-0.038
	(2.21)	(−3.15)	(−2.67)
DISCOUNT	18.212	8.285	8.069
	(9.65)	(3.06)	(2.99)
ELECTION	−0.348	−0.325	−0.329
	(−0.80)	(−2.93)	(−2.97)
ADM	−0.211	−0.031	−0.031
	(−0.51)	(−0.25)	(−0.25)
TRANS	−0.785	0.461	0.365
	(−0.60)	(1.20)	(0.94)
DEBT	−0.010	0.016	0.016
	(−0.67)	(1.53)	(1.54)
GRANTS	−0.185	−0.063	−0.066
	(−2.31)	(−0.87)	(−0.90)
REGIME			−1.190
			(−2.40)
Adj. R2	0.58	0.65	0.66
N	1,261	1,141	1,141

[a]Dependent Variable: TAX. See Appendix 2 for definitions of variables.
[b]T-ration in parentheses.

ment's overall fiscal situation is not statistically relevant for taxation, although the coefficients have the expected sign.[39]

IV. THE IMPACT OF POLITICAL REGIMES ON TAXES

Given the factors specified in Section III, the question we shall address now is whether the nature of the political regime has any impact on the

[39] The results do not change in any significant way if we modify the definitions of the variables. The GDP share of imports (or exports) alone is positively associated with taxation. Using the cumulative changes of chief executive as a direct indicator of the government's discount rate also does not change anything. Finally, no substantive change occurs if we use other hazard functions (e.g., proportional, estimated only with the gowth of per capita income or with regional dummy variables).

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government's extractive capacity. The analysis that follows is based on a classification of 108 countries as democracies or dictatorships for every year between 1970 (or the first year data on taxation were available) and 1990. Democracies were defined as regimes in which the most important political offices are filled by contested elections. Practically, this implies that the chief executive and the legislature are elected, that elections are contested, and that they are held even if the incumbents have a chance to lose.[40] Regimes that did not meet at least one of these conditions were classified as dictatorships.

The issue is whether political regimes have a causal effect on taxation. Over the period from 1970 to 1990, the time for which comparable data on taxation are available for several countries, democracies tended to collect more taxes than dictatorships: on the average, 26.7 percent of the GDP in democracies and 17.8 percent in dictatorships. "Rich" democracies (countries with per capita income above $7,000) also collected more taxes than "rich" dictatorships: 31.5 percent against 18.5 percent of the GDP. Although the difference is considerably smaller, the same is true if we compare democracies and dictatorships in "poor" countries: 20.2 percent against 17.8 percent of the GDP.[41]

Perhaps more importantly, if we estimate the model represented by equation 2 with a dummy variable for political regime (coded 1 for dictatorships and 0 for democracies), we find, again, that the level of taxes tends to be lower in authoritarian regimes than in democracies: column 3 in Table 1 shows that, on average, the level of taxation in dictatorships is over 1 percent below the level of taxation in democracies. Thus, even after controlling for other factors that also affect taxation, such as per capita income, the relative size of the agricultural and mining sectors, the country's openness to foreign trade, and the occurrence of elections, we find that taxes tend to be higher in democracies, suggesting that democracy does have a positive impact on the government's extractive capacity. Yet, we should proceed with care when drawing this kind of inference from the data. If there is some factor that simultaneously affects the variable whose impact we want to study (in this case different political regimes) and the outcome we want to explain (levels of taxation), then inferences may be tainted by selection bias.

In general, selection bias occurs when some determinants of the outcome in which we are interested are correlated with the variable whose

[40] See Mike Alvarez et al., "Classifying Political Regimes," *Studies in International Comparative Development* 31 (Summer 1996).

[41] If we exclude OECD countries, taxes are still higher in democracies than in dictatorships, although the difference is considerably smaller: 18.8 percent against 17.7 percent.

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TABLE 2
LEVEL OF TAXATION BY PER CAPITA INCOME AND REGIME TYPE[a]

Per Capita Income (LEVEL)	*Proportion Dictatorship*	*All*	*Dictatorship*	*Democracy*	*(N)*
<1,000	0.9401	14.39	14.69	9.60	334
1,001–2,000	0.7857	19.59	19.90	18.45	294
2,001–3,000	0.5914	18.24	19.31	16.76	208
3,001–4,000	0.5460	19.59	19.42	19.79	163
4,001–5,000	0.6379	21.19	20.27	22.81	58
5,001–6,000	0.4211	23.83	17.05	28.76	57
6,001–7,000	0.2766	27.73	24.18	29.09	47
7,001–8,000	0.1500	25.61	19.75	26.64	40
8,001–9,000	0.1026	26.36	18.44	27.26	39
>9,000	0.0107	32.14	16.78	32.31	373
ALL	0.5239	22.00	17.81	26.61	1,613

[a] See Appendix 2 for definitions of variables and sources.

effect we want to assess. As explained by Heckman, this correlation may be due to observable or unobservable factors, and depending on which is the case, different statistical techniques are required.[42] In the present case, one possible source of selection bias would be the level of economic development. Because economic development may affect both the likelihood of observing democratic regimes and the level of taxation, the observed higher levels of taxes in democratic regimes could be exclusively due to the mix of regimes at each level of development and not to any feature of the regime that affects taxation. In fact, as Table 2 demonstrates, both the number of democratic regimes and the levels of taxation increase with economic development, as measured by real per capita income.[43]

Had economic development been the only factor related simultaneously to taxation and political regimes, and thus the only cause of se-

[42] James J. Heckman, "The Microeconomic Evaluation of Social Programs and Economic Institutions," in *Chung-Hua Series of Lectures by Invited Eminent Economists*, no. 14 (Taipei: The Institute of Economics, Academia Sinica, 1988).

[43] These numbers are in line with what has been found by others. Seymour Martin Lipset was probably the first to provide statistical evidence for the connection between economic development and democracy, a finding that has proven to be quite robust. Lipset, "Some Social Requisites of Democracy: Economic Development and Political Legitimacy," *American Political Science Review* 53 (March 1959). See also Adam Przeworski and Fernando Limongi, "Modernization: Theories and Facts," *World Politics* 49 (January 1997). Empirical support is also strong for the relationship between economic development and levels of taxation. See H. H. Hinrichs, *A General Theory of Tax Structure Change during Economic Development* (Cambridge: Harvard Law School International Tax Program, 1966); and Vito Tanzi, "Quantitative Characteristics of the Tax Systems of Developing Countries," in David Newbery and Nicholas Stern, eds., *The Theory of Taxation for Developing Countries* (New York: Oxford University Press, 1987).

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lection bias, controlling for this factor through standard regression techniques would have been sufficient for producing satisfactory estimates of the causal impact of political regimes on taxation.[44] This would be a case of selection on observables. To believe, however, that economic development is the only factor simultaneously affecting taxation and political regimes is also to believe either that nothing else affects political regimes and taxation or that the researcher is able to specify and observe all possible mechanisms that affect them. Since those beliefs seem unreasonable, we must assume that selection bias can be the product of unobservable factors and can be the factor behind what we observe. In this case, standard regression analysis fails to produce correct estimates of the effects in which we are interested.[45] And for this reason, selection bias needs explicitly to be taken into account.

There are several ways to take selection bias into consideration when selection is assumed to occur on unobservables. The analysis in this work is based on the procedure proposed by Heckman, which essentially consists of two steps.[46] The first is to estimate through probit the parameters of the selection equation, that is, the equation which represents the process whereby a country is observed as a democracy or as a dictatorship. This yields the probability that a country that is observed as one regime will be observed as the other regime, given the postulated selection mechanism. This probability (the hazard rate) can be used as an estimate of the bias introduced into the sample by the process that makes us observe countries as authoritarian or democratic. The second step is to estimate separately the determinants of the level of taxation for each regime, including the hazard rate as an additional variable to control for the selection process and the bias it may introduce. For this reason, the coefficients of the other determinants of taxation are unbiased. Because this procedure produces unbiased estimates of the determinants of taxation in each regime, it also provides the necessary components for evaluating the effects of political regimes on taxation under identical exogenous conditions. Under these circumstances, inferences about the effects of regimes on taxes can then be made.

[44] Heckman (fn. 42), 17–19.

[45] Table 3 suggests that some unobserved factor besides the level of economic development may be operating with respect to taxation and political regimes. Although the mix of regimes is different at each level of per capita income, and taxes are higher at each level, taxes are higher in dictatorships when per capita income is low, and higher in democracies when per capita income is high. Although it would be desirable to attempt to specify and observe the factors that account for this pattern, this is not necessary for the more limited purpose of assessing whether political regimes have any causal impact on the level of taxation.

[46] James J. Heckman, "The Common Structure of Statistical Models of Truncation, Sample Selection, and Limited Dependent Variables and a Simple Estimator for Such Models," *Annals of Economic and Social Measurement* 5, no. 4 (1976).

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TABLE 3
DETERMINANTS OF DEMOCRACY AND DICTATORSHIP
PROBIT ESTIMATION[a]

Variable	*Coefficient*	*Std. Error*	*t-ratio*	*Prob|t|≥x*
Constant	12.5340	46.0700	0.272	0.78556
STRA	2.0510	0.2015	10.177	0.00000
STRD	−2.7996	0.2290	−12.228	0.00000
ODRP	1.6863	0.7600	2.219	0.02650
ETHNIC	−0.0192	0.0055	−3.486	0.00049
NEWC	−0.4338	0.3957	−1.096	0.27302
BRITCOL	−3.3698	0.4116	−8.187	0.00000
MOSLEM	0.0282	0.0056	5.020	0.00000
CATH	−0.0260	0.0051	−5.131	0.00000
LEVEL	−0.046979	0.009632	−4.877	0.00000
TRADE	−0.0083523	0.004244	−1.968	0.04906
AGRIC	0.0047885	0.01365	0.351	0.72569
MINERAL	0.0052564	0.008581	6.125	0.00000
DISCOUNT	−6.0941	1.758	−3.467	0.00053
ELECTION	−0.089822	0.1836	−0.489	0.62464
ADM	−0.18209	0.1722	−1.058	0.29027
TRANS	−0.12300	0.4498	−0.273	0.78451
DEBT	−0.010626	0.006670	−1.593	0.11114
GRANTS	−0.17848	0.05660	−3.154	0.00161
AFRICA	−4.0197	46.06	−0.087	0.93045
SOASIA	−9.1068	46.05	−0.198	0.84325
EASIA	−6.2330	46.05	−0.135	0.89234
OECD	−10.190	46.06	−0.221	0.82490
LATAMER	−6.3984	46.06	−0.139	0.88951

[a] See Appendix 2 for definitions of variables and sources.

Table 3 presents the coefficients that express the relative impact of factors such as economic development, historical legacies, and international conditions on the probability that in any given year a country will be observed as a dictatorship. On the basis of these coefficients, we can compute, for each observation, the likelihood that a dictatorship (or democracy) in the sample of countries analyzed here will be observed as a democracy (or dictatorship) at any point in time, *given* the determinants of political regimes.[47] As explained above, this probability becomes a variable (LAMBDA) that serves as an instrument for political regimes in the reestimation of the model of taxation presented earlier.

[47] Table 3 includes the main variables identified in the cross-national literature on the determinants of political regimes. See Kenneth A. Bollen, "Political Democracy and the Timing of Development,"

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Table 4 presents estimates of the determinants of taxation in democracies and dictatorships, including the variable LAMBDA. As the significant coefficient for this variable indicates, sample selection bias operates for both political regimes. We can see in the table that, despite several similarities, there are also significant differences in the determinants of taxation in democracies and dictatorships. Transaction costs, for example, are important in both regimes: taxes increase with the relative importance of foreign trade and decrease with the size of the agricultural sector in democracies and dictatorships. The level of economic development, as indicated by per capita income, however, is relevant only in democratic regimes, whereas the relative importance of mineral production affects taxation only in dictatorships. As discussed before, this last fact suggests that a large mineral sector may represent an alternative source of revenue for the government, thus allowing it to alleviate the tax burden for the general population. If this is true, a large mineral sector is indeed a base upon which authoritarian governments extract revenue at a relatively low cost, although not in the form of taxation.

Taxes in both dictatorships and democracies are higher when governments feel relatively insecure in office, lending support to the idea that governments tend to plunder when the rate at which they discount the future is relatively high. Taxes are significantly higher when transitions to democracy take place, as the positive and significant coefficient for the variable TRANS under democracies indicates.[48] New democracies, therefore, are not impaired in their ability to collect taxes. On the contrary, as far as taxation is concerned, young democracies do have a significant credit.

American Sociological Review 44 (August 1979); Kenneth A. Bollen and Robert W. Jackman, "Economic and Noneconomic Determinants of Political Democracy in the 1960s," *Research in Political Sociology* (1985); Phillips Cutright, "National Political Development: Measurement and Analysis," *American Sociological Review* 28 (April 1963); Seymour Martin Lipset, Kyoung-Ryung Seon, and John Charles Torres, "A Comparative Analysis of the Social Requisites of Democracy," *International Social Science Journal* 136 (1993); D. E. Neubauer, "Some Conditions of Democracy," *American Political Science Review* 61 (December 1967); and Tatu Vanhanen, *The Process of Democratization* (New York: Crane Russack, 1992). Although the concern here is not with the estimation of the determinants of regimes per se, it is worth noting that the results reported in Table 3 conform with the main findings of this literature: we are more likely to observe authoritarian regimes when countries experienced transitions to authoritarianism in the past (STRA), when the number of democratic regimes in a country's own region increases (ODRP), and when countries have a higher Moslem population (MOSLEM). Democracies, in turn, are more likely to be observed in countries that have experienced at least one democratic period (STRD), that are ethnically homogeneous (ETHNIC), that were not independent as of 1950 (NEWC), that were British colonies if not independent as of 1950 (BRITCOL), and that have a large Catholic population (CATH). Together, these estimates predict correctly 90 percent of the democratic regimes observed in the sample and 95 percent of the dictatorships.

[48] Because transition years are coded as the regime that emerges in that year, TRANS under democracies indicates a new democracy.

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TABLE 4
DETERMINANTS OF TAXATION WITH SELECTION IN DEMOCRACIES AND DICTATORSHIPS
(COUNTRY-SPECIFIC FIXED-EFFECTS ESTIMATION, CORRECTED FOR AUTOCORRELATION)

Variable[a]	*Democracies*	*Dictatorships*
LEVEL	0.0624	−0.0291
	(4.113)[b]	(−0.519)
TRADE	0.0309	0.0743
	(2.432)	(5.036)
AGRIC	−0.2061	−0.1653
	(−3.096)	(−3.518)
MINERAL	−0.0780	−0.0475
	(−1.641)	(−2.295)
DISCOUNT	11.7330	8.0048
	(3.533)	(1.443)
ELECTION	−0.2293	−0.3526
	(−1.541)	(−1.670)
ADM	0.1150	−0.0421
	(0.745)	(−0.151)
TRANS	2.4006	0.1909
	(2.003)	(0.109)
DEBT	0.0223	0.0507
	(1.391)	(3.066)
GRANTS	0.0976	−0.0945
	(0.636)	(−1.055)
LAMBDA	−3.9199	−3.9274
	(−2.396)	(−2.111)
Adj. R^2	0.86	0.68

[a] Dependent Variable: TAX. See appendix for definition of variables and sources.
[b] T-ratio in parentheses.

We may say that elections affect taxation significantly under both democracies and dictatorships if we are willing to accept a relatively large margin of error. That elections have an effect on the level of taxes in democracies should not be surprising. The surprising finding here is that authoritarian regimes may also be vulnerable to the elections they promote, even when, as it is often the case, the level of uncertainty regarding their outcome is relatively low. This, therefore, would suggest that authoritarian regimes are sensitive even to the very low levels of uncertainty with which they are sometimes forced to live. Elections represent moments of true weakness for the regime and, in some cases, may produce quite unexpected results.

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TABLE 5
LEVELS OF TAXATION (% OF GDP) UNDER DEMOCRACIES AND DICTATORSHIPS ASSUMING DIFFERENT CONDITIONS ARE ENDOGENOUS TO POLITICAL REGIMES

Conditions Endogenous to Regime	*Democracies*	*Dictatorships*
None	10.84	9.96
Bargaining power (ELECTION, ADM, TRANS)[a]	10.85	9.97
Fiscal requirements (GRANTS, DEBT)	10.70	10.14
Bargaining power, fiscal requirements	10.71	10.15
Government's discount rate (DISCOUNT)	11.22	9.71
Discount + bargaining + fiscal	11.09	9.91
Transaction costs (LEVEL, TRADE, AGRIC, MINERAL)	14.94	9.14
All	14.69	9.19

[a] See Appendix 2 for definitions of variables and sources.

Finally, the government's fiscal requirement is also an important determinant of taxation in dictatorships, but not in democracies. As indicated by the positive and significant coefficient of the variable DEBT in Table 4, the higher the pressure represented by servicing the foreign debt, the higher the level of taxes collected by authoritarian governments. The irrelevance of this factor for democracies may be the product of the several moratorium decisions undertaken by some of the new Latin American democracies in the mid-1980s.

This leaves us still faced with the central issue of whether a political regime causes differences in the government's extractive capacity as expressed by levels of taxation. To answer that question, we need to know if there are differences in the level of taxation across regimes when the other conditions that also affect taxation are identical from one regime to the other. Table 4 provides all the necessary elements for making this comparison. First, we have unbiased estimates of the relative impact of the determinants of taxation in each regime. We also know the exogenous conditions that obtained in each country, including the per capita income, the relative size of the agricultural sector, the government's discount rate, and so on. By applying the regime-specific coefficients to the conditions experienced in each country, we can calculate the level of taxation a given country would experience as a democracy and as a dictatorship given its specific conditions. We can then assess whether being a democracy or a dictatorship affects the country's overall level of taxation.

Table 5 presents the average level of taxation we would observe if all countries were characterized by identical conditions (given by all or a

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combination of variables included in equation 2), but by different political regimes. In the first row, all of the conditions are assumed to be exogenous with respect to regime. We observe that the difference in the average level of taxes between the two regimes is less than one percentage point, too small to be of any substantive significance.

Not all conditions, however, are exogenous with respect to political regimes. If the government's discount rate, bargaining position, or fiscal requirement are specific to each regime, then it does not make sense to assume that the values we observe for these variables under one regime could be the same under the other. Counterfactuals that assume that these conditions are the same across regimes are no longer feasible.[49] We need, therefore, to calculate the level of taxation that would be obtained under each regime when some of the determinants of taxation are regime-specific.[50]

Unfortunately there is no way to determine systematically those conditions that are endogenous to a regime. We could, of course, test for these conditions but would then fall into infinite regress: we would have to test the variables used for the test and so on. All we can do is to see whether treating different factors as exogenous makes any difference for the calculated average level of taxation under each regime. The remaining rows of Table 5 allow us to do precisely this. Each of them treats a different set of factors as endogenous to political regimes. We can see that the difference in taxation across regimes becomes significantly large only when the variables that indicate transaction costs are treated as endogenous to political regimes. Those, however, are the factors that can most plausibly be treated as exogenous since they reflect structural characteristics of each country. We can say, therefore, that the causal impact of political regimes on taxes is likely to be negligible; it certainly is much smaller than the observation of taxes actually collected in each regime would lead us to believe. Thus, the reason we observe a large difference in taxation in favor of democratic regimes has to do not with any inherent characteristics of this type of regime, but with the factors that make us observe countries as democracies or as dictatorships. Once we take these factors into consideration, we find that the

[49] Adam Przeworski and Fernando Limongi, "Selection, Counterfactuals and Comparisons" (Manuscript, New York University, 1996).

[50] When all variables are assumed to be exogenous, we calculate the figures in Table 5 by computing $t_{jit} = \mathbf{x}_{it}\mathbf{ß}_j$, where j = 0 (democracies) or 1 (dictatorships), $\mathbf{x}_{it}$ is the vector of variables in equation 2 for country i at year t, and $\mathbf{ß}_j$ is the vector of coefficients for these variables under each regime, as reported in Table 4. When some variables $\mathbf{x}'$ in $\mathbf{x}$ are assumed to be endogenous to regimes, we compute $t_{jit} = \mathbf{x}_{it}\mathbf{ß}_j + \bar{\mathbf{x}}'_j\mathbf{ß}_j$, where x now represents a subset of the variables in equation 2 and $\bar{\mathbf{x}}'_j$ is the average for each regime of the variables in equation 2 that are considered endogenous with respect to regime.

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government's extractive capacity is not much stronger in one regime than in the other.

V. Conclusion

Instead of concluding in the face of contradictory evidence that regimes do not matter for economic policy and outcomes, we should try to specify empirically the substantive areas for which and the conditions under which they may or may not matter. This article has attempted to do so by examining whether the observed difference in favor of democracies regarding the level of taxation is due to any inherent characteristics of democratic regimes; in other words, whether we have reasons to worry about the ability of new democracies to raise the tax revenue necessary to resume growth.

The findings reported here are unambiguous: there are no grounds for believing that democracies are less capable of collecting taxes than dictatorships. Although taxes are higher in democracies than in dictatorships, we should not infer that this is due to any inherent feature of democratic regimes. Once we control for the conditions that make us observe countries as one regime type or the other, and conjure up counterfactuals in which countries experience conditions that are identical in every respect except for their political regime, we observe that the difference in the level of taxes between democracies and dictatorships all but disappears. Concerns about the inability of democratic regimes to collect taxes are therefore unfounded. The image of governments in new democracies being compelled into a tax-and-spend syndrome, or into the traps of economic populism due to the very logic of democratic competition, is not warranted. As far as the level of taxes collected by the government is concerned, observed differences across countries are not due to the fact that some are under a democracy and others under a dictatorship.

It is not that the two regimes do not matter for taxation. Even though taxation under democracies and dictatorships is influenced by broadly similar factors, there are also notable differences from one regime to the other. Thus, the relative size of the agricultural sector and of foreign trade, the rate at which the government discounts the future, and the occurrence of elections affect the level of taxes that governments collect in democracies and dictatorships. Per capita income, however, affects taxation only in democracies, whereas the pressure of servicing foreign debts only affects the level of taxes in dictatorships. Most significantly, democracies collect more taxes in the years they are

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inaugurated, suggesting that, contrary to the fears of some analysts, new democratic regimes do not suffer from chronic weakness when it comes to extracting resources from society. We see, therefore, that although there are factors that influence differently the level of taxes collected by the government in each regime, regime type does not affect the overall level of taxation. Democracies are not any less capable than dictatorships of extracting taxes from society.

Appendix 1: Data Coverage

The sample was determined by the availability of comparable data on taxation. The countries included in the analysis and the period for which data were available follow.

Africa

Benin (1976–79), Botswana (1971–86), Burkina Faso (1973–87), Burundi (1973–86), Cameroon (1975–90), Central African Republic (1981–89), Chad (1972–90), Congo (1970–83), Egypt (1975–89), Ethiopia (1972–86), Gabon (1973–90), Gambia (1972–90), Ghana (1972–82), Guinea (1988–90), Guinea-Bisau (1986–89), Ivory Coast (1980–90), Kenya (1972–90), Lesotho (1971–90), Liberia (1974–86), Madagasgar (1972–90), Malawi (1971–90), Mali (1975–88), Mauritania (1975–88), Mauritius (1973–90), Niger (1976–80), Nigeria (1972–87), Rwanda (1973–90), Senegal (1970–79), Sierra Leone (1974–90), Somalia (1972–78), South Africa (1972–90), Sudan (1972–82), Swaziland (1971–89), Tanzania (1972–85), Togo (1977–87), Tunisia (1972–90), Uganda (1972–86), Zaire (1971–89), Zambia (1972–89), and Zimbabwe (1976–90).

Western Hemisphere

Bahamas (1979–86), Barbados (1972–89), Belize (1981–90), Canada (1971–89), Costa Rica (1972–90), Dominican Republic (1972–90), El Salvador (1970–81), Guatemala (1972–89), Haiti (1980–87), Honduras (1970–81), Jamaica (1975–85), Mexico (1972–90), Nicaragua (1970–86), Panama (1973–90), Trinidad and Tobago (1976–82), United States (1972–90), Brazil (1970–90), Chile (1972–82), Colombia (1971–89), Ecuador (1973–90), Guyana (1970–85), Paraguay (1972–90), Peru (1970–80), Suriname (1975–86), Uruguay (1972–82), and Venezuela (1970–90).

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ASIA

Bangladesh (1973–89), India (1974–90), Indonesia (1972–82), Iran (1974–90), Japan (1970–90), Jordan (1983–90), Malaysia (1972–83), Nepal (1972–86), Pakistan (1973–90), Philippines (1972–90), Singapore (1970–90), South Korea (1970–90), Sri Lanka (1970–90), Syria (1986–90), and Thailand (1972–90).

EUROPE

Austria (1970–90), Belgium (1970–90), Bulgaria (1987–90), Denmark (1970–90), Finland (1972–90), France (1972–90), Germany (1970–90), Greece (1972–90), Iceland (1972–90), Ireland (1970–90), Italy (1973–90), Luxembourg (1970–90) Malta (1972–89), Netherlands (1973–90), Norway (1972–90), Romania (1985–89), Spain (1970–90), Sweden (1970–90), Switzerland (1970–90), Turkey (1970–90), United Kingdom (1970–90).

OCEANIA

Australia (1970–90), Fiji (1970–90), New Zealand (1970–90), Papua New Guinea (1975–90), Solomon Islands (1981–88), Vanuatu (1984–89).

APPENDIX 2: DEFINITIONS OF VARIABLES[51]

REGIME. Dummy variable that is coded 1 for dictatorships and 0 for democracies. Transition years are coded as the regime that emerges.

TAX. Central government total tax revenue as a percentage of the GDP at factor cost, both in local currency.

LEVEL. Real GDP per capita, 1985 international prices, Chain index.

TRADE. Share of exports and imports in GDP in 1985 international dollars.

AGRIC. Agriculture, value added, percentage of the GDP.

MINERAL. Exports of metals and minerals, percentage of the total exports of goods and NF services.

[51] Alvarez et al. (fn. 40) is the source for REGIME, ELECTION, ADM, TRANS, STRA, STRD, ODRP, NEWC, and BRITCOL. World Bank (fn. 37) is the source for TAX, AGRIC, MINERAL, DEBT, and GRANTS. Penn World Tables, release 5.6, is the source for LEVEL and TRADE. *The World in Figures: Editorial Information Compiled by* The Economist (Boston: G. K. Hall, 1988), and Vanhanen (fn. 47) are the sources for ETHNIC. *Leksykon Pan'stw S'wiata* 1993–94 (Warsaw: Real Press, 1993) is the source for MOSLEM and CATH.

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DISCOUNT. The probability that the chief executive will be removed from office, given the length of his/her tenure in office; the rate of economic growth (defined by the rate of change of LEVEL); and the number of changes of chief executive accumulated in each country since 1950. The probability is estimated through a duration model assuming a Weibull distribution.

ELECTION. Dummy variable that is coded 1 for the years in which a presidential or a legislative election occurred.

ADM. Dummy variable that is coded 1 for the years in which at least one change in the chief executive occurred.

TRANS. Dummy variable that is coded 1 for the years in which a regime transition occurred.

DEBT. Ratio of service on the outstanding public foreign debt to exports.

GRANTS. GDP share of grants to the central government.

STRA. The sum of past transitions to authoritarianism since 1950. If a country experienced at least one transition to authoritarianism before 1950, STRA was coded 1 in 1950.

STRD. The sum of past transitions to democracy since 1950. If a country experienced at least one transition to democracy before 1950, STRD was coded 1 in 1950.

ODRP. Percentage of democratic regimes in the current year (other than the regime under consideration) in the region to which the country belongs.

ETHNIC. The percentage of the population of the largest ethnic group, measured in the year for which data were available (roughly 1976 to 1985).

NEWC. Dummy variable that is coded 1 for every year for countries that were not independent as of 1950.

BRITCOL. Dummy variable that is coded 1 for every year for countries that had been a British colony prior to independence.

MOSLEM. Percentage of the population that is Moslem.

CATH. Percentage of the population that is Catholic.

AFRICA, SOASIA, EASIA, LATAMER, OECD. Dummy variables that are coded 1 for countries in Africa, South Asia, East Asia, Latin American, and the OECD, respectively.

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WHY DO FORMAL AMENDMENTS FAIL?
An Institutional Design Analysis

By CHRISTOPHER P. MANFREDI and MICHAEL LUSZTIG*

THE process of constitutional modification has become an increasingly important element of world politics. From Eastern Europe, Latin America, and South Africa to the United Kingdom, Belgium, Spain, Australia, and the European Union, political regimes are either in the process of constituting and reconstituting themselves or engaged in critical discussions about constitutional change. It is thus imperative to examine the success and failure of constitutional modification systematically, particularly given some of the surprising, and counterintuitive, anecdotal observations about the process of constitutional change in some regimes. For example, Stephen Holmes and Cass Sunstein report that in Eastern Europe at least two of the core concepts of modern constitutionalism—broad democratic participation and the constitutional protection of rights—may actually impede the transition from authoritarianism to constitutionalism.[1] Indeed, they argue that procedurally rigid and highly participatory amendment processes, as well as reliance on the judicial protection of rights, may undermine the capacity of political regimes to achieve constitutional change.

The purpose of this paper is to construct a systematic, institution-oriented explanation for the breakdown of constitutional politics that can be generalized across political regimes, including those in which constitutionalism is well established.[2] In our view, the first step in this

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[1] Holmes and Sunstein, "The Politics of Constitutional Revision in Eastern Europe," in Sanford Levinson, ed., *Responding to Imperfection: The Theory and Practice of Constitutional Amendment* (Princeton: Princeton University Press, 1995), 294–301.

[2] The explanation we propose is the product of two independent research projects. Earlier versions of the model are found in Michael Lusztig, "Constitutional Paralysis: Why Canadian Constitutional Initiatives Are Doomed to Fail," *Canadian Journal of Political Science* 27 (December 1994); and Christopher P. Manfredi, "Institutional Design and the Politics of Constitutional Modification: Understanding Amendment Failure in the United States and Canada," *Law & Society Review* 31, no. 1 (1997).

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direction is to recognize that constitutional politics not only encompasses symbolic purposes but also serves the instrumental interests of its participants by permitting them to entrench, preserve, and promote their policy objectives. The aim of the present analysis is to understand the conditions under which the instrumental objectives of participants in the politics of constitutional modification threaten to overload the institutional capacity of the amending process to resolve key issues. We argue that two factors are crucial: the intensity of elite and mass preferences surrounding constitutional proposals and the structure of the amending process itself. Furthermore, we trace both of these factors back to institutional variables.

The analysis is undertaken in the context of a comparative case study of two countries, Canada and the United States. Two principal features of these cases justify the comparison: (1) Canada and the United States are representative of two influential models of liberal constitutionalism; and (2) both have recently experienced highly visible and unsuccessful attempts to achieve formal constitutional amendments. Regarding the first feature, the U.S. Constitution has influenced numerous others, including Switzerland, the Philippines, and many Latin American countries.[3] Similarly, Canada is the oldest codified embodiment of the British tradition, which is the basis of governance in the Commonwealth countries. In addition, Canada is representative of the large number of countries that have recently included bills of rights in their constitutions. This provides an important contrast to the long tradition of judicial interpretation of constitutional rights in the United States. Consequently, because Canada and the United States together encompass the principal elements of tradition and innovation in liberal democratic practice, they provide appropriate case studies to introduce the model and illustrate its generalizability.

The second comparable feature of these cases—failure to achieve formal amendments—in Canada involved the Meech Lake and Charlottetown Accords and in the United States involved the Equal Rights Amendment (ERA). The comparability of these particular cases of failed

[3] If we consider aspects such as judicial review and federalism, the scope of influence is even greater, encompassing, for example, Germany and Japan. See Louis Henkin and Albert J. Rosenthal, eds., *Constitutionalism and Rights: The Influence of the United States Constitution Abroad* (New York: Columbia University Press, 1990). The U.S. influence has also been important for constitutional reform movements in Israel. See Gary Jeffrey Jacobsohn, *Apple of Gold: Constitutionalism in Israel and the United States* (Princeton: Princeton University Press, 1993), 11. The U.S. influence on Canadian constitutionalism, especially recently, is also significant. See Christopher P. Manfredi, "The Judicialization of Politics: Rights and Public Policy in Canada and the United States," in Keith Banting, George Hoberg, and Richard Simeon, eds., *Degrees of Freedom: Canada and the United States in a Changing Global Context* (Montreal and Kingston: McGill-Queen's University Press, 1997).

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constitutional amendment lies not so much in their substance (although we argue later that they share some features in common) but in the remarkably similar political dynamic that led to their defeat. In each case the proposed amendments enjoyed elite support and began the process of ratification with high levels of public approval, with the result that there was strong early momentum toward ratification. However, opponents were ultimately able to block ratification. The question we pose in this article is whether there is a common, generalizable explanation for the rapid and quite unexpected change in the political fortunes of these proposed amendments.

The paper is divided into four parts. We begin by setting out an institutional design model of constitutional modification in order to identify the structural factors underlying constitutional conflicts that overload the amending process. The second part of the paper uses this model to construct a systematic explanation for the recent pattern of success and failure in the politics of constitutional modification in Canada. In the third part, we extend the model to the United States, examining the failure of the Equal Rights Amendment to the U.S. Constitution. Finally, the paper concludes with some general reflections on the future utility of this approach.

An Institutional Design Model of Constitutional Modification

Political actors in all regimes operate within an institutional framework of procedural and substantive rules. Although these rules are usually contained in constitutional texts, not all of these texts are equally authoritative. Moreover, even where a constitutional text possesses a high degree of authority, it may inaccurately describe or only partially capture the range of rules that control political behavior.[4] To the extent that constitutional rules, regardless of their source, constrain political behavior, however, these constraints operate systematically to favor particular sets of outcomes over others. For this reason, the politics of constitutional modification can be conceptualized as a competitive game of institutional design in which the objective is to alter the range of possible policy outcomes by modifying procedural or substantive rules.[5] In-

[4] Walter F. Murphy, "Constitutions, Constitutionalism and Democracy," in Douglas Greenberg et al., eds., *Constitutionalism and Democracy: Transitions in the Contemporary World* (New York: Oxford University Press, 1993), 7–10.

[5] George Tsebelis, *Nested Games: Rational Choice in Comparative Politics* (Berkeley: University of California Press, 1990), 98.

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stitutional design at the constitutional level is a particularly powerful form of rule modification because it is generally difficult to reverse once accomplished, and its impact reverberates throughout lower-level (subconstitutional) rules. From this perspective, constitutional modification is an instrument for changing the dynamics of political power and altering the status of competing interests.

The primary objective of this paper is to explain what we have called *amending process overload*, operationally defined as the inability to achieve successful completion of constitutional modification as a result of key actors' making incompatible and intractable demands during the process of constitutional negotiation. We suggest that constitutional modification can take two forms: *incremental* and *comprehensive*. The former entails the simple attempt to alter discrete aspects of a constitution's original design without changing the fundamental nature of the regime in question. Put simply, incremental reforms do not result in the redistribution of constitutional power or status.[6]

By contrast, comprehensive modifications are those that address "the very nature of the political community" and concern "the identity and fundamental principles of the body politic."[7] In operational terms, comprehensive reform can be defined as constitutional modification that seeks to alter substantively a nation's institutional framework rather than merely implement procedural changes to existing institutions. These changes are comprehensive to the extent that they are multilateral—negotiated among all groups recognized as legitimate players in the constitutional bargaining game.[8] Thus, for example, under federal constitutions comprehensive change requires, at a minimum, the opportunity for all subnational governments to have input into the decision-making process. In addition, since some procedural changes—such as guaranteeing the right to vote to a previously excluded group of citizens (for example, racial minorities or women)—may have significant insti-

[6] Examples of incremental reform would include most of the post-Reconstruction amendments to the U.S. Constitution (with the exception of the Seventeenth and Nineteenth), Canadian constitutional amendments dedicated to reconciling the retention of the representation-by-population principle with changing demographics and limited space within the House of Commons, and recent amendments allowing New Brunswick and Newfoundland to make important changes to the administration of education policy.

[7] Peter H. Russell, *Constitutional Odyssey: Can Canadians Be a Sovereign People?* (Toronto: University of Toronto Press, 1992), 75.

[8] It is possible to conceptualize comprehensive modification that does not take the form of formal, multilateral amendments. Important judicial decisions, such as *Marbury v Madison* (1803) and *Brown v Board of Education* (1954), are examples. However, beyond recognizing this caveat, we are not explicitly concerned with this type of comprehensive constitutional modification. For more on judicially mandated comprehensive reforms, see Walter F. Murphy, "Merlin's Memory: The Past and Future Imperfect of the Once and Future Polity," in Levinson (fn. 1).

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tutional consequences, comprehensive reform includes constitutional changes aimed at modifying the status of and relationship among social groups and/or governmental actors. In short, comprehensive reforms are redistributive insofar as they are purposefully designed to skew resource distribution and policy outcomes in favor of one or more social (or state-level) actors at the expense of others.[9]

Amending process overload is the product of a superficially simplistic phenomenon: too much and overly intense competition among groups to control or alter the redistributive impact of comprehensive constitutional modification. More specifically, we argue that conditions are ripe for amending process overload where (1) constitutions are difficult to amend, (2) the institutional structure allows for the existence of numerous constitutional actors (both state and societal), and (3) the existing constitutional language and judicial practice provide for a wide range of interpretation.

The proximate cause of amending process overload is what might be termed *redistributive indeterminacy*. Redistributive indeterminacy entails uncertainty on the part of constitutional actors about the redistributive impact of constitutional modification. This redistributive indeterminacy has three sources: what we call *amending process rigidity*, *interpretive fluidity*, and *institutional inclusiveness*. In turn, redistributive indeterminacy manifests itself in two ways, through what we refer to as *regulative rule demands* and *interpretive rule demands*. We illustrate the argument schematically (Figure 1), then operationalize the variables in sequence, and ultimately illustrate the operation of the model.

Amending Process Rigidity

Amending process rigidity refers simply to how difficult it is to amend a constitution. According to Donald Lutz, constitutional amendment strategies can be arrayed along a spectrum ranging from simple legislative supremacy (least rigid) to one combining multiple legislative paths and required referenda (most rigid).[10] We suggest that where levels of amending process rigidity are low, the consequences associated with redistributive indeterminacy are also low; that is, miscalculations in one

[9] The Reconstruction Amendments to the U.S. Constitution, the reconstituting of post-1945 Germany, and the constitutional engineering currently under way in South Africa could be described as comprehensive constitutional modifications. Similarly, most of the constitutional reforms embodied in Canada's 1982 Constitution Act and the failed Meech Lake and Charlottetown Accords can be included in this category.

[10] Lutz, "Toward a Theory of Constitutional Amendment," *American Political Science Review* 88 (June 1994), 363. Of course, changing the means by which constitutions are amended affects amending process rigidity. Thus, in Canada, for example, amending process rigidity must be analyzed in the context of significant changes to the amending formula in 1982.

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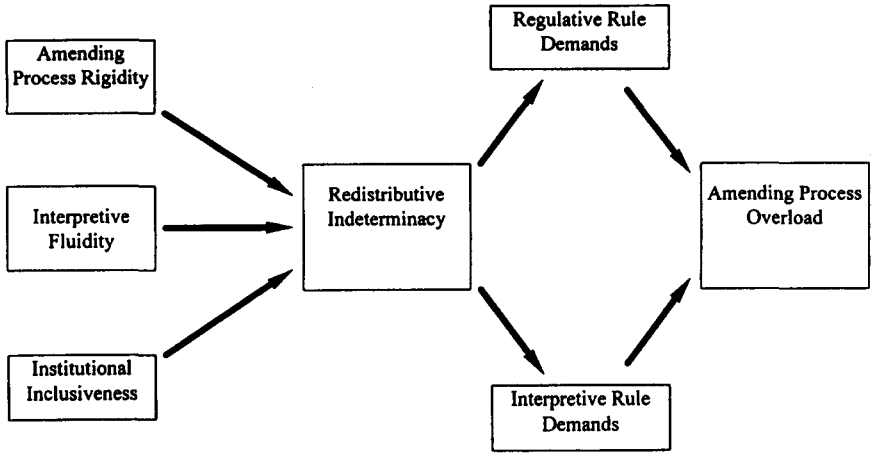

FIGURE 1
THE AMENDING PROCESS OVERLOAD MODEL

round of constitutional bargaining can be corrected in the next. The analogy is public policy formation, where bargaining over policy may at times be intense, but it is rarely paralytic. The fluidity of the legislative process lowers the stakes at any given point in the process. This fluidity is the reason, for example, that it is relatively easy for coalitions to form in the legislative process. Issues are often logrolled, with various groups supporting one another's objectives with the tacit understanding that their own objectives will be supported.

By contrast, high levels of amending process rigidity increase the importance of redistributive indeterminacy. Mistakes are more difficult to redress. Rather than imitating the fluidity of the legislative process, high amending process rigidity ensures that each amendment is treated as a discrete issue. As a result, not only are groups less willing to compromise on their demands for the sake of maintaining policy coalitions, but these groups are also more likely to make their demands in unambiguous language that is invulnerable to interpretations that may run counter to the sponsoring group's intent.

INTERPRETIVE FLUIDITY

Interpretive fluidity is a second variable impacting on redistributive indeterminacy, and it is a function of how much scope constitutional language leaves for interpretation. Where language is unambiguous, as in the U.S. Nineteenth Amendment, which gives women the right to vote, there is very little latitude for interpretation. By contrast, the due

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process clause of the Fifth Amendment provides room for a broad range of legislative and judicial action. Indeed, judicial interpretation is especially important because it alone has the status of constitutional law; it is therefore more authoritative and less easily challenged or altered than other types of interpretation. Thus, while political actors must be concerned with future interpretation by all sources, it is judicial interpretation that contributes most to interpretive fluidity.

Obviously interpretive fluidity contributes to redistributive indeterminacy. Occasionally certain groups, typically those that have historically not had political status and that have fared poorly in the public policy arena, will seek ambiguous language in proposed constitutional amendments. They do this as an innocuous means of entrenching constitutional objectives, subsequently hoping for the chance of favorable interpretations through the courts. The aboriginal movement in Canada attempted this strategy in both the 1981 Patriation Round and the 1992 Charlottetown Round. By contrast, groups that are more firmly established in the political process are more likely to seek the entrenchment of carefully worded amendments, even if it is more difficult to secure agreement on such amendments. This is because groups that enjoy a high degree of political status do not want to risk losing gains already made to the vagaries of interpretively fluid constitutional language.

Of course, as mentioned, interpretive fluidity is more contentious where constitutions feature high levels of amending process rigidity. It is especially contentious in circumstances where there is a large and indeterminate number of constitutional actors—thus the importance of our third variable.

Institutional Inclusiveness

The relevance of the institutional inclusiveness variable is based on the presumption that constitutionally entrenched institutions reflect the interests of those who brought about their creation. As Peter Gourevitch notes, because institutions are never power neutral, the rules and procedures by which they operate "always help or hinder some groups over others."[11] The same logic holds for constitutional rules, which, after all, are the basis of most political institutions. Thus constitutional rules can be said to reflect the victory of a set of collective interests over all other interests within society. By this logic, battles over institutional creation or reform may be conceptualized as a form of constitutional pluralism.

[11] Gourevitch, "Keynesian Politics: The Political Sources of Economic Policy Choice," in Peter A. Hall, ed., *The Political Power of Economic Ideas: Keynesianism across Nations* (Princeton: Princeton University Press, 1989), 101.

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Insofar as it is adversarial, constitutional pluralism resembles legislative pluralism. Proposals for constitutional change are put forward, and groups mobilize in support of and in opposition to such changes. There is, however, at least one significant difference between constitutional and legislative pluralism: barriers to entry in the constitutional arena tend to be higher. The logic of pluralism suggests that the number of latent groups that can mobilize to seek to influence the legislative policy agenda is limitless.[12] By contrast, the constitutional arena is confined to those who can mobilize for special or enhanced constitutional status, which is characterized by "recognition in a hierarchy that lets those who hold it stake preferential claims on the political resources of the state or on the political process itself."[13]

Enhanced constitutional status offers groups entrée to the constitutional arena. The number and types of groups that can claim enhanced constitutional status is conditioned by constitutional rules in two ways. First, rules can specifically privilege certain groups. A federal constitution, for example, privileges national and subnational governments to the exclusion of other collectivities. This explicit recognition of status provides a power base that privileged groups seek to protect and, where possible, expand. The history and development of Canadian constitutional politics can be understood from such a perspective.[14]

The second means by which rules condition enhanced constitutional status is that they have the potential to provide market niches for aggressive new political entrepreneurs. Indeed, an externality associated with the construction of certain types of rules is that such rules may provide unanticipated opportunities for groups to acquire enhanced constitutional status. The extent to which constitutional rules provide such niches is the extent to which they are what we call *inclusive*.

Constitutional rules vary in their potential to provide market niches. Indeed, some rules, which bestow status based on class or territorial considerations, are *noninclusive* insofar as there is an inherent limit to the number of collectivities that conceivably could mobilize for status. The European estate system, federalism, and such quasi-constitutional

[12] See especially Robert Dahl, *Who Governs? Democracy and Power in an American City* (New Haven: Yale University Press, 1961).

[13] Ian Brodie, "The Market for Political Status," *Comparative Politics* 28 (April 1996), 253. See also Alan C. Cairns, "Constitutional Minoritarianism in Canada," in Ronald L. Watts and Douglas M. Brown, eds., *Canada: The State of the Federation, 1990* (Kingston: Institute of Intergovernmental Relations, 1990), 73.

[14] See Alan C. Cairns, "The Governments and Societies of Canadian Federalism," *Canadian Journal of Political Science* 10 (December 1977); Roger Gibbins, "Federal Societies, Institutions, and Politics," in Herman Bakvis and William Chandler, eds., *Federalism and the Role of the State* (Toronto: University of Toronto Press, 1987).

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features as social corporatism are examples of noninclusive structures. They privilege, respectively, social classes, subnational governments, and owners of factors of production. In each case, the potential number of actors is limited; leaving little or no opportunity for new groups to acquire enhanced constitutional status.

By contrast, codified bills of rights, for example, may provide market niches for limitless numbers of political entrepreneurs, and as such, their introduction may stimulate the mobilization of status-seeking groups. However, because the number of status-seeking groups is indeterminate, the redistributive impact of comprehensive constitutional reform may also be difficult to gauge where constitutions feature inclusive institutions. Moreover, where institutional inclusiveness occurs in conjunction with our other independent variables—amending process rigidity and interpretive fluidity—redistributive indeterminacy becomes more problematic.

Regulative and Interpretive Rule Demands

Redistributive indeterminacy conditioned by the three factors described above manifests itself in two types of demands by constitutional actors: *regulative rule demands* and *interpretive rule demands*. Regulative rule demands are direct attempts to reduce the ambiguity of redistributive indeterminacy. They constitute, in other words, attempts to entrench specific policies, as opposed to broad principles, in a constitution. Regulative rules determine policies across "a limited set of policy outcomes" and are "generally indistinguishable in form from the rules contained in ordinary legislation."[15] They are, in the simplest possible terms, new constitutional rules designed to privilege the interests of sponsoring groups.

Constitutional actors also make demands for interpretive rules to clarify the impact of existing rules and principles. In other words, they seek to ensure that the redistributive impact of rules and policies will be as favorable as possible, and while interpretive rules tend not to be as directly valuable as regulative rules, they are still important. Indeed, the constitutional strategy of more marginal constitutional actors may well entail a two-stage process: entrench ambiguous language into the constitution as a preliminary step and then seek interpretive modifications to ensure favorable redistributive impact.

Both regulative and interpretive rule demands are attempts by groups to overcome redistributive indeterminacy by limiting the maneuverability of judicial decision makers. These demands are typically

[15] Stephen M. Griffin, "Constitutionalism in the United States: From Theory to Politics," in Levinson (fn. 1), 40.

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contentious and invite counterdemands by groups that are hostile or even indifferent to such demands. Such counterdemands are most likely, we argue, where a large number of constitutional actors perceive a one-shot opportunity to amend an interpretively fluid constitution. This perception, in turn, increases the likelihood that the politics of constitutional modification will generate multiple demands for regulative and interpretive rules designed to ensure particular policy outcomes. The result is a degree of overload that may cause the amending process to collapse. In the sections that follow, we test this proposition in preliminary fashion by examining why Canada succeeded in implementing a highly conflictual set of amendments in 1982 but failed to do so in 1987–90 and in 1992. We then explore the proposition further by applying it to another recent case in a similar federal system—the failed Equal Rights Amendment to the United States Constitution.

Constitutional Modification and Amending Process Overload in Canada

There can be little doubt that Canada in the 1990s suffers from amending process overload. The 1992 Charlottetown Accord, consisting of sixty amendments (many of which were merely frameworks for future negotiations), is ample evidence of this problem. But Canada is illustrative of the amending process overload problem for another reason as well. In just one decade Canada experienced three rounds of megaconstitutional politics with a wide variety of outcomes. In 1981–82 Canada was able to effect comprehensive constitutional change (albeit to the dissatisfaction of many). In 1987–90 the country came within a "roll of the dice," to use former Prime Minister Brian Mulroney's metaphor, of successful amendment with the Meech Lake Accord. Finally, the Charlottetown Accord, which emerged from a more open negotiation process, did not survive a national referendum in October 1992.[16] While this overwhelming defeat in 1992 seems to have quelled the apparently insatiable passion of Canadian political elites for constitutional change, at least in the short and medium term, the varied outcomes provide an excellent opportunity to illustrate the amending process overload model.[17]

[16] Together, the Meech Lake and Charlottetown failures have arguably brought Canada to the brink of a constitutional and political crisis as serious as any that has faced a constitutional democracy since 1861. Indeed, on October 30, 1995, Quebecers voted to remain within Canada by a vote of 50.56 percent to 49.44 percent.

[17] Much scholarly ink has been spilled over explaining the failures of 1992 and 1987–90. See, for example, Richard Simeon, "Why Did the Meech Lake Accord Fail?" in Watts and Brown (fn. 13);

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Given that federal and provincial governments in Canada had tried unsuccessfully for over fifty years to agree upon an indigenous amending formula for the constitution, it is somewhat counterintuitive to suggest that the pre-1982 era was free from the amending process overload phenomenon. Indeed, first ministers' conferences in 1927, 1935, 1964, and 1971 failed to produce lasting agreement on an amending formula. By contrast, a uniquely imaginative and determined prime minister, Pierre Trudeau, was able to effect significant constitutional change within fourteen months of his October 1980 threat to patriate the constitution unilaterally. This would suggest that the constitutional deadlock of the pre-1982 era was largely illusory.

Indeed, the pre-1982 era, at least in the twentieth century, meets none of our criteria for amending process overload. With respect to amending process rigidity, for example, the written constitution was actually fairly flexible. In the 115 years between 1867 and 1982 the constitutional text underwent comprehensive, or redistributive, reform on sixteen occasions (1870, 1871(2), 1873, 1893, 1905, 1912, 1915, 1930, 1940, 1949, 1960, 1964, 1965, 1975, 1982), for a rate of one comprehensive reform every 7.19 years.[18] Moreover, these reforms were not enacted disproportionately during the early years of Confederation, when the British Parliament was under relatively little pressure to con-

Patrick J. Monahan, *Meech Lake: The Inside Story* (Toronto: University of Toronto Press, 1991), 238–59; Raymond Breton, *Why Meech Failed: Lessons for Canadian Constitutionmaking* (Toronto: C. D. Howe Institute, 1992); Kenneth McRoberts and Patrick Monahan, eds., *The Charlottetown Accord, the Referendum, and the Future of Canada* (Toronto: University of Toronto Press, 1993); Curtis Cook, ed., *Constitutional Predicament: Canada after the Referendum of 1992* (Montreal: McGill-Queen's University Press, 1994); Lusztig (fn. 2).

Substantive explanations argue that the two accords attempted either too much or not enough; or that they constituted too much or not enough of a compromise. Procedural explanations assert that the negotiation process was either too closed (Meech Lake) or too open (Charlottetown). Finally, political explanations focus either on the persuasive skills of political elites or on mass rejection of elite opinion. Although each of these explanations undoubtedly contains a grain of truth, they suffer the defect of being issue oriented and context specific. Thus, while the failure of Meech Lake is attributed to a closed negotiation process that could not produce a true compromise, Charlottetown's failure is explained as the result of an unduly open process that produced an unprincipled compromise that political elites could not sell to a disenchanted public. Two entirely different explanations, in other words, are posited for similar breakdowns in the politics of constitutional modification.

[18] In order to maintain consistency, we have used the 1982 amending formulas as the benchmark for comprehensive constitutional change in Canada. Thus, we consider any amendment that would have required either the unanimity or general (two-thirds of the provinces, which together have at least one-half of the population of all the provinces combined) amending formula as indicators of comprehensive reform. By this definition, comprehensive reforms through 1982 were the Manitoba Act (1870); Constitution Act (1871); British Columbia Terms of Union (1871); Prince Edward Island Terms of Union (1873); Statute Law Revision Act (1893); Alberta Act, Saskatchewan Act (1905); Ontario Boundaries Extension Act, Quebec Boundaries Extension Act, Manitoba Boundaries Extension Act (1912); Constitution Act (1915); Constitution Act (1930); Constitution Act (1940); Newfoundland Act (1949); Constitution Act (1960); Constitution Act (1964); Constitution Act (No. 2) (1975); Constitution Act (1982).

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form to the demands of constitutional actors in Canada. Seven of these amendments occurred after the Statute of Westminster (1931), for a rate of one reform every 7.29 years through 1982. The flexibility of constitutional modification prior to 1982 is even more apparent if one takes into account nontextual modification to constitutional rules, which occurs in Canada through the development and evolution of constitutional conventions.[19] By contrast, since 1982 Canada has been unable to enact any comprehensive reforms to its written constitution, despite a series of constitutional conferences dedicated to comprehensive reform in 1983, 1984, 1985, 1987(2), and 1992.

The pre-1982 Constitution, at least in the twentieth century, also falls short on our interpretive fluidity variable. To be sure, in the early years of Confederation, the imprecise language regulating the federal/provincial division of powers under sections 91 and 92 clearly was interpretively fluid, and it consequently generated significant levels of constitutional litigation.[20] However, the nature of federalism litigation was such that its consequences and thus its redistributive impact were not entirely beyond the control of the governments affected by federalism review.[21] Indeed, the flexibility of the amending process allowed the federal and provincial governments to respond rather easily to adverse constitutional decisions. For example, in 1940 both levels agreed to revise section 91 to give the federal government constitutional jurisdiction over unemployment insurance after the Judicial Committee of the Privy Council declared that this power was ultra vires the federal government.[22]

Finally, with respect to institutional inclusiveness, the pre-1982 constitution can be characterized as noninclusive. Only leaders of the provincial and federal governments were recognized as legitimate constitutional actors (and indeed, the prevailing constitutional issues were

[19] See Andrew Heard, *Canadian Constitutional Conventions: The Marriage of Law and Politics* (Toronto: Oxford University Press, 1991). In 1981 the Supreme Court held that the "total constitution" of Canada consists of constitutional convention (historical practice) plus constitutional law (text and judicial interpretation). However, the Court also held that only constitutional *law* is enforceable by courts. See *Reference re Resolution to Amend the Constitution* [1981] 1 S.C.R. 753.

The degree of amendment flexibility in Canada is also apparent in comparison with the U.S. Constitution, which enacted fifteen constitutional amendments during its first 115 years of existence (1789–1904). However, these amendments included a ten-amendment package in 1791 and the three Reconstruction Amendments added between 1865 and 1870. Moreover, two of the amendments could be described as incremental (Eleventh, Twelfth). Consequently, one might argue that the U.S. Constitution experienced only *two* comprehensive amendments between 1789 and 1904.

[20] See Peter Russell, Rainer Knopff, and F. L. Morton, *Federalism and the Charter: Leading Constitutional Decisions* (Ottawa: Carleton University Press, 1989).

[21] Patrick Monahan, *Politics and the Constitution: The Charter, Federalism and the Supreme Court of Canada* (Toronto: Carswell/Methuen, 1987), 221–44.

[22] *A.-G. Canada v A.-G. Ontario (Unemployment Insurance Reference)* (1937) A.C. 355.

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those concerning the division of powers and, by extension, the amending formula). The first move toward inclusiveness came with Prime Minister Trudeau's patriation initiative, which included a charter of rights and freedoms dedicated to the protection of individual rights. In the spring of 1981, with Trudeau's unilateral initiative facing stiff opposition from the "gang of eight" provinces opposed to his patriation package, the Trudeau government amended its draft charter of rights and freedoms to include specific provisions protecting the rights of two prominent extragovernmental groups—aboriginals, with sections 25 and 34 (later section 35), and women, with section 28. Although Trudeau would later seek to remove most of these concessions from the final constitutional package upon securing agreement for his patriation initiative from seven of the eight recalcitrant provinces in November 1981, sustained pressure from aboriginal and women's groups ensured the retention of special provisions. These provisions, along with section 27 (preserving and enhancing the multicultural heritage of Canada), section 23 (protecting minority language education rights), and the judicial interpretation of the right to equality provision (section 15) allowing specific and affirmative action rights for categories enumerated in that section, as well as for "discrete and insular minorities," have provided entrée to the constitutional arena for any number of nongovernmental groups. The effects of this were seen in the aftermath of the Patriation Round.

The Meech Lake era provides the first illustration of the amending process rigidity that has characterized the Constitution since 1982. The 1982 amending formula allows for two types of amendment. Some require the support of the federal parliament and two-thirds of the provinces that collectively constitute 50 percent of the country's population; others require unanimity. Moreover, all amendments must be ratified by the requisite legislatures within three years of the date that the first legislature ratifies. Meech Lake would have made the formula even more rigid, by increasing the number of amendment categories that require unanimous ratification.

The 1982 amending formula intensifies amending process rigidity in two ways. First and most obviously, it gives all provinces a veto over many comprehensive reforms. Thus, all constitutional actors are aware that constitutional change will be more difficult to achieve than in the past. The result is an incentive for elites to front-load their constitutional demands into one omnibus package, rather than deal with issues sequentially and discretely. This incentive is one reason the Meech Lake Accord was so unpopular in Western Canada. Western leaders

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were not successful in securing concrete amendments for reform of the Senate during that round. The explicit promise that Senate reform would be the next item on the constitutional agenda, coupled with an even more restrictive amending formula under Meech Lake, caused many Westerners to believe that Senate reform would forever be frozen out of the constitutional process. To many, in the colorful words of Eugene Forsey, Senate reform after Meech would be as likely as Forsey's becoming "the Archbishop of Canterbury, the Pope or the Dalai Lama."[23] Similarly, supporters of aboriginal rights, such as Premier Frank McKenna of New Brunswick, and, more famously, Elijah Harper of the Manitoba legislative assembly, believed that the exclusion of aboriginal issues from the accord compromised the likelihood that aboriginal issues would be dealt with successfully in the future.

Second, the 1982 amending formula intensifies amending process rigidity through the inclusion of a sunset clause (section 29(2)), whereby all amendments must be ratified by the appropriate legislatures within three years of the date that the first legislature ratifies. Section 29(2) therefore creates an incentive for provinces to delay ratification in the hopes of extracting concessions from other provinces (or the federal government) with a stronger intensity of preference for the amendment under consideration. Provinces that delay ratification can thus make public demands for concessions that are very difficult to retract. As such, bargaining that once took place in private—so-called elite accommodation—is subjected to a second, more public round of bargaining in the ratification stage. It is in this second stage that the original agreement can potentially break down. The Charlottetown Accord process institutionalized this second round of bargaining through the use of a nationwide series of referenda.

The post-1982 era also witnessed a number of problems associated with interpretive fluidity. The preponderance of these problems involved constitutional litigation designed to put jurisprudential flesh on the bare bones of the new Charter of Rights and Freedoms. It is thus logical to assume that latent and potential constitutional demands were postponed in the early charter years until groups had a chance to determine, at least minimally, the redistributive effects of the charter. However, there was one area of interpretive fluidity that created almost immediate demands: aboriginal rights.

The Constitution Act (1982) entrenched a weak commitment to aboriginal rights under sections 25 and 35. However, the act left the

[23] *Globe and Mail*, May 4, 1987.

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precise meaning and function of aboriginal rights undefined, to be worked out in a series of constitutional conferences held between 1983 and 1987. Thus, aboriginal leaders came to the bargaining table in 1983 with regulative rule demands regarding a native charter of rights and freedoms and an aboriginal veto.[24] In addition, they made a series of interpretive rule demands dedicated to clarifying the operation of native self-government; these included the powers to choose citizenship and form of government, determine their own institutions, make laws to govern their own communities, be exempt from federal and provincial taxation, and enjoy full mobility across provincial or national borders.[25] These interpretive rule demands have been fairly consistent since 1983. Indeed, it was intransigence on the part of many governmental actors that led to the failure of constitutional conferences on aboriginal rights in 1983, 1984, 1985, and 1987. It also explains why, in June 1990, aboriginal leaders rejoiced in the irony that Elijah Harper—a native Indian—was afforded the opportunity to scuttle the Meech Lake Accord single-handedly.

The Meech Lake era was not as famous for the problems associated with institutional inclusiveness. Indeed, while many groups, such as those representing aboriginals, women, and multiethnic Canadians were vocally opposed to the Meech Lake Accord, they had little impact on its substance. This relative lack of impact may be attributable to the fact that many groups singled out for status in the charter were just beginning to recognize the potency of this privilege. However, with the demise of the accord in 1990 and the criticisms leveled at the exclusiveness of the Meech Lake bargaining process, the subsequent Charlottetown Accord provided opportunities for greater input from groups that had previously been shut out of the constitutional negotiating and ratification process.

The Charlottetown Accord affords the best illustration of the problems associated with the amending process overload model. By 1992 the degree of amending process rigidity inherent in the 1982 amending formula was apparent to all constitutional actors. Thus, these actors were not willing, as were the Western provinces during the Meech Lake negotiations, for example, to postpone discussion of their objectives to future constitutional rounds. There was a general consensus that Charlottetown represented a one-shot opportunity to effect constitutional change and that opportunities missed were opportunities

[24] Ibid., March 16, 1983.

[25] Michael Asch, *Home and Native Land: Aboriginal Rights and the Canadian Constitution* (Toronto: Methuen, 1984), 28.

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lost. The result was a host of regulative rule demands that contributed in part to the Charlottetown Accord's ballooning to sixty amendments —a dramatic increase from the seven amendments contained in the Meech Lake Accord. These regulative demands included all of Quebec's demands from the Meech Lake Accord. In addition, there were demands brought by social, labor and business groups, through provincial governments sympathetic to their causes, for a "nonjusticiable" social charter and provisions for internal free trade. Finally, the devolutionist provinces received a Triple-E (elected, effective, and equal) Senate, albeit a watered-down version, as well as the devolution of a number of joint constitutional responsibilities to the provinces.

Interpretive fluidity also played an important role at Charlottetown. The most controversial component of the Meech Lake Accord was the Distinct Society Clause[26]—an interpretive clause designed to influence the reasonable limitations section (section 1)[27] of the Charter of Rights and Freedoms as it pertained to Quebec. At Charlottetown other constitutional actors had become aware of the potential significance of interpretive clauses on the redistributive impact of existing constitutional language. As a result of a number of (often contradictory) interpretive rule demands by constitutional actors, the Charlottetown Accord included a "Canada Clause" that sought to accommodate these demands. The clause was an ensemble that recognized both the equality of the provinces and Quebec's status as a distinct society; it committed Canadians to a respect for individual and collective rights, without specifying how inevitable clashes between the two might be resolved; and it committed Canadians to racial, ethnic, and gender equality, despite the fact that racial and ethnic equality were already guaranteed under section 15 of the charter and gender equality was guaranteed under section 15 and section 28. The only rationale for this redundancy would be to influence the redistributive impact of competing rights claims and hence to privilege certain social cleavages over others. Interpretive fluidity and the interpretive rule demands that resulted also account in large part for the size of the Charlottetown Accord. Indeed, a full twenty-two amendments were dedicated to interpreting "aboriginal rights" as they exist under sections 25 and 35 of the Constitution Act (1982).

Finally, the Charlottetown Accord also conforms to the third source of amending process overload described in our model: it featured un-

[26] The Distinct Society Clause read, in part: "The Constitution of Canada shall be interpreted in a manner consistent with the recognition that Quebec constitutes within Canada a distinct society."

[27] Section 1 reads: "The Canadian Charter of Rights and Freedoms guarantees the rights and freedoms set out in it subject only to such reasonable limits prescribed by law as can be demonstrably justified in a free and democratic society."

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precedented societal input. Indeed, leaders of four aboriginal organizations representing status Indians, nonstatus Indians, Métis, and Inuit were accorded full bargaining rights along with the eleven first ministers and two territorial representatives. In addition, because the accord was subject to informal popular ratification through nationwide referenda, groups such as women, ethnic minorities, minority language education activists, and social and labor leaders had a great deal more input into the constitutional process than they had ever had in the past. Indeed, the referendum campaign witnessed the emergence of a number of societal opinion leaders who evidently carried a good deal of influence with their respective constituents. The referenda resulted in the defeat of the accord in most provinces, as well as at the aggregate level.

In summary, the 1982 Constitution Act was a watershed document in more than the obvious ways, since it changed the institutional context of constitutional modification in Canada. It transformed a flexible and exclusive modification environment without the unique interpretive fluidity of rights-based litigation into one that was inflexible, inclusive, and dominated by judicial interpretation of individual rights. As a result, a much higher level of redistributive indeterminacy became attached to proposals for constitutional modification. The impact of this change became fully apparent during the Charlottetown Accord process, when amending process overload contributed to the accord's ultimate failure.

The failure of the Charlottetown Accord is all the more illustrative of the model articulated here if one considers that the threat of Quebec separatism provided the backdrop for the negotiations leading to the accord. As the accord's supporters pointed out repeatedly, its failure enhanced the possibility of the breakup of Canada. Even this threat, however, was not enough to compel Canadians to vote for ratification. Moreover, as we demonstrate in the next section, the institutional dynamics of amending process overload are not unique to constitutional failure in Canada.

Overload and the Failure of the Equal Rights Amendment

In 1972, after almost fifty years of effort by women's rights advocates, the U.S. Congress sent the Equal Rights Amendment (ERA) to the states for ratification. The amendment provided that "[e]quality of rights under the law shall not be denied or abridged by the United States or any State on account of sex," and it gave Congress the power to enforce this rule through appropriate legislation. By the end of 1972

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ratification of the amendment seemed certain, as thirty of the required thirty-eight states had accepted it. Ten years later, despite Congress's extension of the ratification period, the amendment was dead.

Jane Mansbridge has explained the ERA's failure by observing that "[n]o really controversial amendment has passed [in the United States] since Prohibition was repealed."[28] At first glance, this conclusion is puzzling, given the ERA's long political gestation, during which its supporters successfully built a consensus around the general principle of gender equality.[29] Indeed, a constitutional guarantee of the equal rights of men and women had attracted the support of every U.S. president from Harry Truman to Jimmy Carter. Moreover, the principle of gender equality underlay Title VII of the 1964 Civil Rights Act, which prohibits discrimination on the basis of sex. Finally, Congress itself passed the ERA by large majorities in both the House of Representatives (354 to 23) and the Senate (84 to 8). How, then, were the ERA's opponents able to defeat it?

According to Mansbridge, pro-ERA sentiment diminished as its opponents increasingly argued "that the Amendment would lead to major substantive changes," especially through the intervention of federal judges in their capacity as constitutional interpreters.[30] In terms of the institutional design model of constitutional modification, the ERA became controversial when the focus of the ratification debate explicitly shifted toward its potential redistributive impact within an institutional context that made this impact extremely difficult to predict. As Lutz has demonstrated, the U.S. Constitution has one of the most rigid formal amending processes of any national constitution.[31] Relying on a process characterized by legislative complexity (in which amendments can be proposed only with the approval of two-thirds of both houses of Congress) and the necessity of ratification through a device analogous to approval by popular referendum (in which amendments can be defeated by a negative vote in only twelve of the ninety-nine state legislative chambers),[32] the U.S. had the third lowest amendment rate of the

[28] Mansbridge, *Why We Lost the ERA* (Chicago: University of Chicago Press, 1986), 29.

[29] The story of the ERA in this section relies on Mansbridge (fn. 28); Mary Frances Berry, *Why ERA Failed: Politics, Women's Rights, and the Amending Process of the Constitution* (Bloomington: Indiana University Press, 1986); Janet K. Boles, *The Politics of the Equal Rights Amendment: Conflict and the Decision Process* (New York: Longman, 1979); and Gilbert Y. Steiner, *Constitutional Inequality: The Political Fortunes of the Equal Rights Amendment* (Washington, D.C.: Brookings Institution, 1985).

[30] Mansbridge (fn. 28), 29, 27–28.

[31] Lutz (fn. 10), 363–64.

[32] Every state but Nebraska has a bicameral legislature. Congress could, of course, specify ratification by state conventions, which might change the political dynamics somewhat. However, ratification by state legislatures has been the norm.

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thirty-two nations in Lutz's study.[33] Consequently, it is extremely difficult, if not entirely impossible, to reverse the policy consequences of particular amendments through subsequent formal "counteramendments." Indeed, the repeal of Prohibition in 1933 is the only example of this in U.S. constitutional history.

Perhaps because of the rigidity of its amending process, the U.S. Constitution is also characterized by interpretive fluidity. This characteristic stems not only from the broad, indeterminate language in which most constitutional provisions are written but also from the willingness of courts to exercise the power of judicial review in order to derive more policy-specific rules from those provisions. Although the U.S. Supreme Court established the constitutionality of judicial review in 1803,[34] the interpretive fluidity of the U.S. Constitution has been most evident since 1954. Indeed, between 1889 and 1953 the Court overturned on average about one act of Congress and seven state laws every year. By contrast, since 1954 the judicial nullification rate has approximately doubled to almost two acts of Congress and twelve state laws per year.[35] Especially throughout the 1960s, litigants took advantage of judicial openness toward the Constitution's interpretive fluidity to persuade U.S. courts to participate actively in shaping and administering policy in areas such as zoning and land-use planning, housing, social welfare, transportation, education, and the operation of complex institutions like prisons and mental health facilities.[36] While this may make the document's rigid amending process less burdensome on the constitutional order, the ability and willingness of courts to extend formal rules in unexpected directions heightens redistributive indeterminacy.

Finally, both the rigid amending process and the interpretive fluidity of the U.S. Constitution generate a high degree of institutional inclusiveness. On the one hand, interpretive fluidity provides society-based actors with a wide range of opportunities to institutionalize specific policy preferences by manipulating and transforming formal constitutional rules through litigation. Interpretive fluidity promotes institu-

[33] Lutz (fn. 10), 369. Indeed, the U.S. rate was 95 percent lower than the average rate.

[34] The origin of judicial review in the United States is generally traced to *Marbury v Madison*, 5 U.S. (1 Cranch) 137 (1803). Nevertheless, there is some evidence that these origins date from *United States v Yale Todd* (unpublished, 1794). See P. Allan Dionisopoulos and Paul Peterson, "Rediscovering the American Origins of Judicial Review," *John Marshall Law Review* 18 (Fall 1984).

[35] David M. O'Brien, *Storm Center: The Supreme Court in American Politics*, 2d ed. (New York: W. W. Norton, 1990), 60.

[36] Tinsley E. Yarborough, "The Political World of Federal Judges as Managers," *Public Administration Review* 45 (November1985), 660. Although the 1960s represents a significant increase in the Court's policy-making role, its involvement as an important actor in the policy-making process dates back at least to the end of the nineteenth century. See O'Brien (fn. 35), 245.

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tional inclusiveness by allowing society-based actors to alter the policy impact of constitutional rules without the constraints imposed by the formal amending process. On the other hand, the requirement that ratification succeed in eighty-seven legislative chambers unconstrained by strict party discipline provides numerous points of influence for social actors wary of the policy consequences of proposed amendments. The institutional inclusiveness of U.S. constitutional politics thus provides both incentives to oppose constitutional change *and* the means of carrying out that opposition successfully.

To summarize, high levels of interpretive fluidity and amending process rigidity characterize the institutional nature of constitutional politics in the U.S. Together, these characteristics increase the likelihood, or at least the perception, that constitutional modification will have policy consequences that cannot easily be reversed by subsequent amendments. The institutional inclusiveness of the process ensures that multiple actors will have the opportunity and the incentive to intervene in the process by demanding both interpretive and regulative rules, the objective of which is to confine the policy consequences of proposed amendments within narrow parameters.

This dynamic played an important role in the eventual rejection of the ERA in 1982. Since the early 1920s the supporters of an equal rights amendment have pursued a redistributive strategy. Their initial purpose was to undermine the constitutionality of protective labor legislation that shielded women from allegedly deleterious working conditions and thereby excluded them from many jobs and professions. For almost two decades, a coalition of unions, progressive organizations, and conservatives succeeded in blocking various amendment proposals. Although supporters of an amendment sought to satisfy progressives and the labor movement by attaching regulative and interpretive rules to their proposals that would limit the impact of an equal rights amendment on special benefits for women workers, their efforts remained unsuccessful until the late 1960s, when judicial interpretation of the 1964 Civil Rights Act rendered the question of gender-specific protective measures moot.[37] With the amendment's impact on protective legislation no longer an issue, the National Organization for Women was able to enlist the support of unions and other liberal political organizations for the 1972 ERA.

As in the early 1920s, the redistributive objectives of the ERA were evident in what its proponents hoped to add to the guarantee of equal-

[37] Mansbridge (fn. 28), 10.

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ity already found in the Fourteenth Amendment. First, by modifying the national constitution and providing Congress with potentially new legislative authority with respect to gender issues, the ERA would clearly have nationalized the political struggle over women's rights, permitting the women's movement to utilize its resources more efficiently and giving its victories immediate national scope. Second, and perhaps more importantly, the ERA would have supported the women's movement's existing litigation strategy.[38] Under the Fourteenth Amendment's equal protection clause, this strategy had sought to generate a rule that statutory classifications based on gender—like those based on race—are inherently suspect, thereby making such classifications subject to the generally fatal "strict scrutiny" standard of constitutional review.[39] The best that women were able to accomplish, however, was to raise the standard of review for gender-based classifications from a weak "rational basis" test to a somewhat stronger intermediate level of review.[40] One of the principal objectives of the ERA, therefore, was to overturn existing constitutional law by making gender-based classifications suspect. In fact, three justices of the U.S. Supreme Court used this objective in *Frontiero v. Richardson* to justify, at least in part, their opinion that the Court should leave its existing approach to gender classifications intact.[41]

The ERA's redistributive implications became particularly apparent following the Supreme Court's abortion decision in late January of 1973. In that decision—*Roe v. Wade*—the Court revolutionized U.S. constitutional law by holding that women could claim a virtually unrestricted right to abortion during the first two trimesters of pregnancy. What made this decision particularly controversial was the Court's inability to demonstrate convincingly that either the general right to privacy or the specific right to abortion could be found in the express language or "manifest tenor" of the Constitution. For opponents of abortion, the ERA was problematic because it could be interpreted as providing a more solid constitutional foundation for the right to abortion. This interpretation would have complicated legislative efforts by the pro-life movement to limit the impact of *Roe*.[42] In addition, the ERA

[38] Karen O'Connor, *Women's Organizations' Use of the Courts* (Lexington, Mass.: Lexington Books, 1980).

[39] According to this standard of review, a classification may be upheld only if it is *necessary* to achieve a *compelling* state interest.

[40] The rational basis test simply requires that statutory classifications be reasonable in view of their purpose. The intermediate level of review generally requires a "close fit" between a classification and an "important' (but not compelling) state interest.

[41] *Frontiero v Richardson*, 411 U.S. 167 (1973).

[42] Steiner (fn. 29), 63–66.

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made further "feminist" constitutional gains possible. If the Court could extract a right to abortion from the distantly relevant language of the First, Third, Fourth, and Fifth Amendments, ERA opponents asked, what might it do with an amendment directly linked to the interests of the women's movement?

To the ERA's opponents, the decision in *Roe* clearly signaled the impossibility of predicting the amendment's policy consequences. They were thus able to use *Roe* to attack the ERA as the prelude to a more comprehensive feminist litigation campaign, the outcome of which would be to eliminate existing protections for women while imposing new obligations on them such as compulsory military service in combat roles. Congressional supporters of the ERA responded to the opposition's strategy by attempting to attach interpretive rules to the amendment that would foreclose the policy consequences that the ERA's opponents were using to attack it. These attempts were opposed by ERA supporters, who argued that interpretive rules would undermine the amendment's symbolic value and make it more difficult to mobilize activists at the state level who were unwilling to expend energy on anything less than fundamental constitutional modification. Moreover, from their perspective, these interpretive rules contributed to a different kind of redistributive indeterminacy by making it less likely that the ERA would have its intended positive policy impact. In the final analysis, the institutional dynamics of the U.S. amending process—multiple actors engaged in a one-shot competition over an interpretively fluid constitutional rule—generated a high level of redistributive indeterminacy that transformed virtually certain ratification into failure when the process collapsed over the issue of interpretive rules designed to reduce that indeterminacy.

To return to Mansbridge's observation: the ERA was controversial because of its redistributive indeterminacy. To a large degree, that indeterminacy was a direct result of an amending process characterized by high levels of amending process rigidity, interpretive fluidity, and institutional inclusiveness. Nevertheless, indeterminacy need not have been fatal; it could have been mitigated by a set of interpretive rules designed to assure opponents that the ERA would not have the redistributive consequences they feared. But this would have reversed the direction of indeterminacy, a fact recognized by the ERA's proponents, who therefore rejected such rules. The result was failure through amending process overload, although in a slightly different way than was the case with the Charlottetown Accord. In the Canadian case, amending process overload resulted when state-based actors agreed to virtually every regula-

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tive and interpretive rule demand proposed for the amendment package. In the ERA case the amendment could not survive the regulative and interpretive rule demands of its opponents because the ERA's supporters would not accommodate those demands. In both cases, however, redistributive indeterminacy generated demands that could not be satisfied.

CONCLUSION

In this paper we have offered a systematic explanation for the failure of comprehensive constitutional modification by examining the institutional environment in which such change takes place. To reiterate our model: we suggest that an institutional environment characterized by amending process rigidity, interpretive fluidity, and institutional inclusiveness contributes to a high level of indeterminacy about the redistributive impact of proposed comprehensive modifications. We further argue that constitutional actors respond to this phenomenon by expressing regulative and interpretive rule demands in order to reduce the level of indeterminacy. The result is constitutional failure through amending process overload. The Canadian case offers an excellent illustration of the model: contrary to what one might expect, comprehensive constitutional modification has become more difficult precisely because the success in 1982 defined a specific amending formula and laid the foundation for institutional inclusiveness and interpretive fluidity. The cross-national utility of the model is suggested by its applicability to the failure of the ERA in the United States.

There are other notable cases of failed constitutional amendments that are consistent with our model. Australia's rigid amending formula (which requires passage by the federal Parliament and ratification by referendum of a majority of voters in a majority of states, as well as a majority of voters nationwide) has long conspired to make amendments in that country difficult to achieve.[43] Of forty-two proposed constitutional amendments, only eight have been passed; among the most important failures was a proposed bill of rights that was defeated in 1988. In addition to the rigidity of the amending formula, commentators have noted that the vehemence with which interest groups mobilize to protect their constitutional interests is an important aspect of Australia's constitutional failures.[44] A more recent example is the failure of

[43] For more on the Australian system, see Brian Galligan, "The 1988 Referendum and Australia's Record on Constitutional Change," *Parliamentary Affairs* 43 (October 1990).

[44] See Joan Rydon, "Australian Constitutional Reform," *Round Table* 308 (1988), 352.

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the abortive U.S. balanced budget amendment. Most noteworthy about this case, perhaps, is the defection of key initial supporters in the conservative movement over fears that interpretive ambiguities might have unintended consequences. Rather than mandating greater fiscal responsibility, these defectors suggested, the amendment could lead to judicially imposed tax increases.[45] This last example suggests that the model's utility will be enhanced even further as the judicialization of politics continues to grow as a global phenomenon.[46]

There is a final way in which the model may be relevant to the analysis of world politics. Although this paper has focused on the breakdown of constitutional modification, the model could be used to examine *successful* cases of comprehensive constitutional change. For example, South Africa completed a constitutional modification that satisfies our definition of comprehensive formal amendment. Similarly, Argentine president Carlos Menem was able simultaneously to enhance the constitutional status of the executive and promote his personal political interests by amending the constitution to permit presidents to seek a second term. Our model would suggest that these successes might be attributed to an amending process whose institutional context approached amendment flexibility, institutional exclusiveness, and interpretive rigidity. Of course, much more analysis would be necessary to test this proposition.

[45] See Hadley Arkes, "Silence the Court," *National Review*, March 20, 1995.

[46] See C. Neal Tate and Torbjorn Vallinder, eds., *The Global Expansion of Judicial Power* (New York and London: New York University Press, 1995).

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FEDERALISM AND CENTRAL BANK INDEPENDENCE
The Politics of German Monetary Policy, 1957–92

By SUSANNE LOHMANN*

TWO channels of political control allow elected politicians to influence monetary policy. First, political threats to the status, structure, or very existence of the central bank may force central bankers to comply with politically motivated demands on monetary policy. Second, politicians may use their powers of appointment to ensure that central bank appointees share their electoral and party-political goals. This paper derives the monetary policy outcomes obtained as a function of the degree of central bank independence (zero, partial, or full) and central bankers' types (partisans or technocrats).

Based on a case study of the 1957 and 1992 institutional changes to the German central banking system and a regression analysis covering the period in between, I argue that the *formal* independence of the system is protected by its embeddedness in the institutions of German federalism and by the federalist components of its decentralized organizational structure. The *behavioral* independence of the German central bank fluctuates over time with the party control of federalist veto points. The Bundesbank is staffed with nonpartisan technocrats who are partially insulated from political pressures.

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I. German Federalism and Central Banking

The Deutsche Bundesbank is celebrated as one of the most independent central banks in the world. The Bundesbank Law of 1957 stipulates that the bank is independent from instructions of the federal government. At first blush, it is surprising that German politicians, who devised this law, would deliberately abdicate power to a central bank, since monetary policy has huge allocative and distributional effects on the wealth and well-being of political constituencies.

One possible explanation is that politicians seek to isolate monetary policy from counterproductive political pressures. If they were to set monetary policy at their discretion, it would be subject to a time-consistency problem, which results in an inflation bias. This problem arises when economic agents in the private sector, expecting the central bank to inflate, write an inflation markup into their nominal contracts, which the central bank is then forced to accommodate so as to avoid depressing employment and output growth.[1] One institutional solution to the time-consistency problem, therefore, has inflation-prone politicians delegate monetary authority to an inflation-averse and independent central bank.[2] Consistent with this explanation, we find that the Bundesbank has in fact presided over some of the lowest inflation rates in the world.

From the perspective of political science, it is unsatisfactory to equate counterproductive political pressures with inflationary pressures arising from the time-consistency problem. In fact, the classic time-consistency problem afflicts benevolent dictators and elected politicians alike; it is not inherent to a democracy—defined as a political system in which political parties compete for votes in regularly scheduled, free elections. Party competition does create counterproductive pressures of its own, however. In contrast to a benevolent dictator, democratic policymakers may have incentives to manipulate the money supply for electoral or party-political purposes.

The *electoral politics hypothesis* of William Nordhaus asserts that incumbents have incentives to expand the money supply prior to elections to stimulate employment and output and thereby increase their chances

[1] Finn E. Kydland and Edward C. Prescott, "Rules Rather than Discretion: The Inconsistency of Optimal Plans," *Journal of Political Economy* 85 (June 1977); Robert J. Barro and David B. Gordon, "Rules, Discretion and Reputation in a Model of Monetary Policy," *Journal of Monetary Economics* 12 (July 1983).

[2] Kenneth Rogoff, "The Optimal Degree of Commitment to an Intermediate Monetary Target," *Quarterly Journal of Economics* 100 (November 1985); Susanne Lohmann, "Optimal Commitment in Monetary Policy: Credibility versus Flexibility," *American Economic Review* 82 (March 1992).

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of reelection.[3] The *party politics hypothesis* of Douglas Hibbs and Alberto Alesina proposes that one political party caters to a constituency with preferences for low inflation, while its competitor represents a constituency that is better off with a high rate of inflation. As a result, monetary growth, inflation, employment, and output vary over time as a function of the party in power.[4] In each case, the political incentives to use monetary instruments for electoral or party-political gain lead to monetary variability that could be reduced or even eliminated if central bankers were insulated from political influences.

The independent status of the Bundesbank can be thought of as an institutional solution to the distortions created by the political vulnerability of monetary policy. If the formal independence of the Bundesbank has meaningful implications for its behavioral independence, electoral and party-political influences on monetary policy in Germany should be less pronounced than elsewhere or even nonexistent.[5] Empirical studies of German monetary policy, however, come to mixed conclusions. Some scholars provide evidence that German monetary policy is subject to electoral influences, others reject the electoral politics hypothesis; some scholars conclude that German monetary policy is subject to party-political influences, others fail to find empirical support for the party politics hypothesis.[6]

[3] William D. Nordhaus, "The Political Business Cycle," *Review of Economic Studies* 42 (April 1975).

[4] Douglas Hibbs, "Political Parties and Macroeconomic Policy," *American Political Science Review* 71 (December 1977); Alberto Alesina, "Macroeconomic Policy in a Two-Party System as a Repeated Game," *Quarterly Journal of Economics* 102 (August 1987).

[5] Empirical measures of legal versus behavioral central bank independence are discussed in Sylvester Eijffinger and Jakob de Haan, *The Political Economy of Central-Bank Independence*, Special Papers in International Economics, no. 19 (Princeton: Princeton University, May 1996).

[6] Bruno S. Frey and Friedrich Schneider, "Central Bank Behavior: A Positive Empirical Analysis," *Journal of Monetary Economics* 7 (May 1981); Byung Hee Soh, "Political Business Cycles in Industrialized Democratic Countries," *Kyklos* 39 (1986); Manfred J. M. Neumann and Susanne Lohmann, "Political Business Cycles in Industrialized Democratic Countries: A Comment," *Kyklos* 40 (1987); Alberto Alesina and Nouriel Roubini, "Political Cycles in OECD Economies," *Review of Economic Studies* 59 (October 1992); Günter Lang and Peter Welzel, "Budgetdefizite, Wahlzyklen und Geldpolitik: Empirische Ergebnisse für die Bundesrepublik Deutschland, 1962–1989," *Jahrbücher für Nationalökonomie und Statistik* 210 (1992); Brian Loynd and R. Michael Alvarez, "Political Control of Central Banking: Is There a Role for Bank Autonomy?" (Manuscript, Duke University, 1992); Roland Vaubel, "Eine Public-Choice-Analyse der Deutschen Bundesbank und ihre Implikationen für die Europäische Währungsunion," in Dieter Duwendag and Jürgen Siebke, eds., *Europa vor dem Eintritt in die Wirtschafts- und Währungsunion* (Europe before the entry into the economic and monetary union), Publications of the Verein für Socialpolitik, new series no. 220 (Berlin: Duncker and Humbolt, 1993); Bernhard Eschweiler and Michael D. Bordo, "Rules, Discretion, and Central Bank Independence: The German Experience, 1880–1989," in Pierre Siklos, ed., *Varieties of Monetary Reforms: Lessons and Experiences on the Road to Monetary Union* (Dordrecht: Kluwer, 1994); David R. Johnson and Pierre L. Siklos, "Political Effects on Central Bank Behavior: Some International Evidence," in Siklos; Helge Berger and Ulrich Woitek, "Searching for Political Business Cycles in Germany," *Public Choice* 91 (April 1997); idem, "How Opportunistic Are Partisan German Central Bankers? Evidence on the Vaubel Hypothesis," *European Journal of Political Economy 13* (1997).

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I argue that there are two reasons why empirical studies of political influences on German monetary policy yield inconclusive results. First, they lack sufficient appreciation of important institutional features of the German central banking system. Second, they fail to control for political factors that affect the degree of (behavioral) central bank independence over time.

Empirical tests of political business cycle hypotheses are generally based on the assumption that the federal government controls the money supply. In other words, they assume that government has not only the *incentives* but also the *ability* to manipulate monetary instruments in pursuit of electoral or party political goals. There are, however, good reasons why it is inappropriate to model the government and the Deutsche Bundesbank as a unitary actor.

The federal government appoints only a minority of the members of the Bundesbank Council, which makes monetary policy decisions by simple majority rule. The organizational structure of the Bundesbank is partially decentralized, consisting of a central headquarters and a system of Land central banks (regional central banks). The presidents of these banks are chosen by the Land (regional state) governments and constitute the majority of the central bank council; the minority, the members of the directorate, are selected by the federal government. (Formally, the president of the Republic appoints all members of the Bundesbank Council. The Land central bank presidents are de jure nominated by the Bundesrat but de facto selected by their respective Land governments; the members of the directorate are nominated by the federal government.)

Federal and Land elections are staggered so that the electoral incentives of federal and Land appointees do not necessarily coincide. Moreover, the major opposition party at the federal level tends to gain votes in Land elections.[7] As a consequence, the federal government often faces opposition in the Länder, and the timing and outcomes of federal elections do not correlate well with changes in the party control of the Bundesbank Council (see Figure 1). In this setting, the appointment powers of the federal government are insufficient to guarantee its control of the money supply. If central bank appointees share the electoral or party-political goals of their respective political principals, then monetary policy outcomes will depend on the majority control of the central bank council rather than the party control of the federal government.

[7] Susanne Lohmann, David Brady, and Douglas Rivers, "Party Identification, Retrospective Voting, and Moderating Elections in a Federal System: West Germany, 1961–1989," *Comparative Political Studies* 30 (August 1997).

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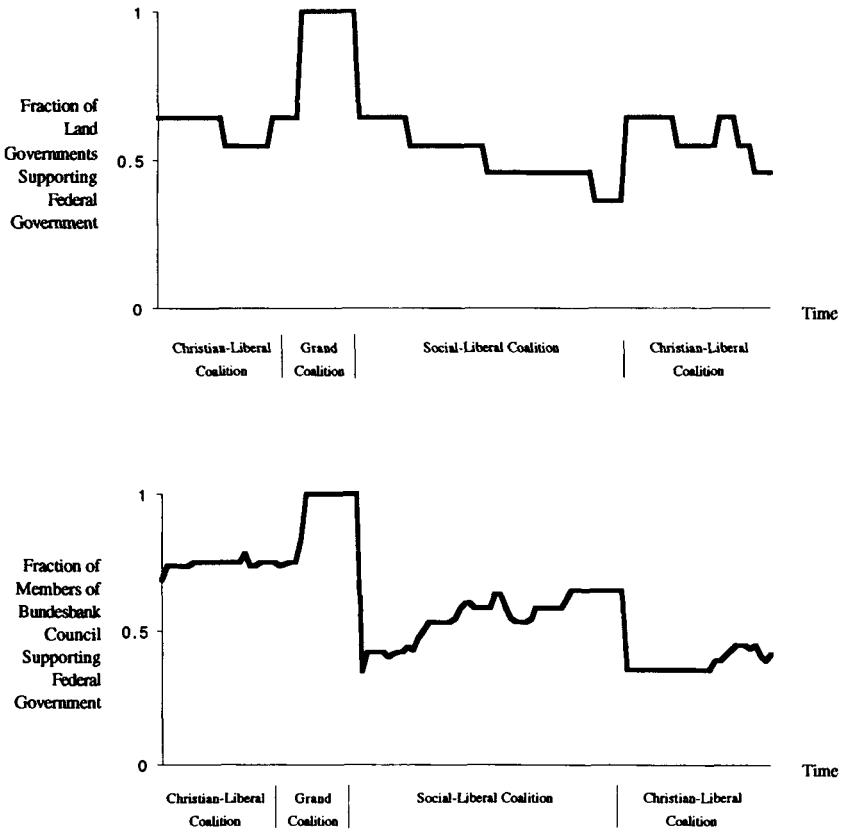

FIGURE 1
PARTISAN SUPPORT FOR THE FEDERAL GOVERNMENT IN THE LÄNDER AND ON THE BUNDESBANK COUNCIL

Roland Vaubel has compiled anecdotal evidence suggesting that the Bundesbank Council accommodates the federal government prior to elections when the government and the council are controlled by the same party but that the council plays an obstructive role when it is majority controlled by the party in opposition at the federal level.[8] After a government coalition of Christian Union Parties (CDU/CSU) and Free Democrats formed in 1982, for example, former Social Democratic chancellor Helmut Schmidt blamed the fall of his Social-Liberal coalition government on the excessively restrictive monetary policy of the early 1980s. His accusations were picked up in a radio commentary:

[8] Vaubel (fn. 6).

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> In the last years of his chancellorship, Schmidt must have experienced the policy of scarce and expensive money as an interest rate bondage. . . . For several years, Schmidt had suspected the strongly federalist central bank council of following a policy of fighting inflation because a majority of Land Central Bank presidents represented CDU/CSU-governed Länder.[9]

A decade later the Bundesbank followed an extremely restrictive monetary policy to cope with the inflationary pressures created by German unification. When Schmidt's successor, Christian Democratic (CDU) chancellor Helmut Kohl, was up for reelection in the fall of 1994, however, the money supply expanded dramatically. The German news magazine *Der Spiegel* commented suspiciously: "In fact, the CDU-dominated central bank council appears to have lost control of the money supply at a time that is strategically most advantageous for reelection purposes."[10]

Implicitly, this discussion—and Vaubel's anecdotal evidence—is predicated on the assumption that elected politicians control the central bank via their powers of appointment: central bankers are partisans who share the electoral or party-political goals of their political principals. Under this scenario, central bank independence does not reduce the political vulnerability of German monetary policy. Whereas a dependent Bundesbank would be responsive to pressures from the federal government, an independent Bundesbank would serve the interests of the party in majority control of the central bank council, which depends on the party control of both the federal and the Land governments.

A plausible alternative to the partisan hypothesis holds that the Bundesbank is a prototypical example of Joseph Schumpeter's "well-trained bureaucracy of good standing and tradition, endowed with a strong sense of duty and a no less strong *esprit de corps*."[11] Unsurprisingly, this is also the self-image of Bundesbank representatives: they are nonpartisan technocrats guided by the Bundesbank's proud ethos of refusing to compromise in matters of inflation.[12]

Elected politicians may deliberately choose to appoint nonpartisan types if they are ex ante better off insulating monetary policy from party-political pressures; and even if they make partisan appointments, they may see their appointees submit to the Bundesbank's stifling insti-

[9] Dieter Balkhausen, *Gutes Geld und Schlechte Politik: Der Report über die Bundesbank* (Good money and bad politics: The report on the Bundesbank) (Düsseldorf: Econ, 1992), 139–40; see also Ellen Kennedy, *The Bundesbank: Germany's Central Bank in the International Monetary System* (New York: Council on Foreign Relations Press, 1991), chap. 3.

[10] "Auf Druck von Oben," *Der Spiegel* 39 (September 26, 1994), 105.

[11] Schumpeter, *Capitalism, Socialism, and Democracy* (New York: Harper, 1942), 293.

[12] Kennedy (fn. 9), 8.

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tutional culture. Once again there is abundant anecdotal evidence about the transformative power of peer group pressures within the Bundesbank—a phenomenon also known as the "Thomas Becket effect."[13]

If central bankers are nonpartisan technocrats, then elected politicians can influence monetary policy by threatening the status, structure, or very existence of the central bank. The degree of central bank independence—or, equivalently, the degree to which the Bundesbank is impervious to political threats—then determines the extent to which central bankers are forced to accommodate electoral or party-political pressures. It is clearly of interest to identify the political factors that might explain such variation over time.

One factor determining the scope of the Bundesbank's discretionary powers is related to the popularity of the chancellor and his economic policies. If the federal government pressures the Bundesbank to accommodate its electoral or party-political demands, the bank can threaten to go public. A public dramatization of the conflict creates political costs—but the popularity of the federal government determines whether these costs are borne primarily by the government or by the Bundesbank. If popular support for the government and its economic policies is already weak, then public resistance on the part of the Bundesbank will further increase the government's vulnerability. If elected officials are popular, however, the bank endangers the legitimacy of its independent status if it publicly quarrels with them. Thus, the government's political capacity to influence monetary policy increases with its popularity.

A second factor determining the degree of central bank independence relates to the institutions of German federalism and central banking. The impact of political pressures on monetary policy depends on the party control of the federal and Land governments, as well as of the two houses of parliament. The Bundesbank is more likely to accommodate political pressures if its independent status would otherwise be undermined. Since the Bundesbank Law is a simple federal law, the government can threaten to change the law to eliminate the formal independence of the Bundesbank, centralize the bank's organizational structure, or even dissolve the Bundesbank institution altogether. As Germany is a parliamentary democracy, legislation proposed by the federal government is typically supported by a majority in the Bundestag, the first house of parliament.

[13] David Marsh, *Die Bundesbank: Geschäfte mit der Macht* (The Bundesbank: Doing business with power) (Munich: Bertelsmann 1992), 33.

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However, the credibility of a legislative threat is limited by the institutions of German federalism. Changes to the Bundesbank Law can be vetoed by the Bundesrat, the second house of parliament. The members of the Bundesrat are delegates of the Land governments, which have a stake in the Bundesbank institution and thus may resist institutional changes—especially changes in the direction of more centralization. The threat of a Bundesrat veto may force the federal government to expend political capital and modify its legislative proposals to ensure passage by the Bundesrat. Even if the veto powers of the Bundesrat are ultimately too weak to prevent a determined government from passing central bank legislation, they can nevertheless create legislative transaction costs and hurdles that delay legislation for many months or even years. At the very least, the Bundesrat veto has the potential to prevent the passage of legislation driven by very short-run political interests.

Given the prominent role of party discipline in German politics, such legislative obstruction is less likely under unified party control of the Bundestag and the Bundesrat, a situation that arises when the parties that form the federal government also control a large number of Land governments. Under those circumstances a threat by the federal government to undermine the Bundesbank's independent status carries some degree of credibility. By contrast, a government that faces a partisan opposition in the Länder and in the Bundesrat is in a weak position to force the Bundesbank to do its bidding. The bank thus enjoys a greater amount of agency discretion under divided party control, while it is forced to accommodate political pressures to a greater extent under unified party control.

When central bank independence derives from a legislative veto point, it is plausible to think of independence as a binary variable—zero or full independence—depending on whether the veto point is on or off. I prefer, however, to conceive of central bank independence as a continuous variable—partial independence—depending on the number of partisan supporters for the federal government in the Bundesrat (or the number of Land governments controlled by the same party or party coalition). The weaker this support, then, the more time, effort, and political capital must be spent by the federal government to ensure that its legislative proposals pass through the Bundesrat; the more the government must compromise and modify its proposals; and the weaker are its threats vis-à-vis the Bundesbank.

Building on a case study of institutional change that demonstrates the impact of the Bundesrat veto point and its partisan control, the regression analysis conducted in this article takes into account a variety

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of political factors that shape German monetary policy. Thus, the analysis has the potential to provide more conclusive evidence for (or against) the notion that German monetary policy is subject to electoral and party-political influences.

II. A Comparison with Federal Reserve System

My analysis of the Bundesbank builds on the scholarly analysis of the United States Federal Reserve System, in which decisions about monetary policy are made by the Federal Open Market Committee (FOMC).[14] The voting membership of the FOMC consists of seven governors and five (of twelve) regional district bank presidents. The governors are political appointees, chosen by the president and subject to Senate confirmation. The appointment of bank presidents, each of whom is nominated by his or her district bank board of directors, is subject to private sector—and, in particular, financial sector—influences. Thus, a majority of the committee is controlled by the federal government; since political appointees constitute a majority on the FOMC and a minority is controlled by regional interests, banker appointees constitute a minority. The district bank presidents tend to prefer more restrictive monetary policies than do the governors. Presidential and congressional powers of appointment and influence appear to create a bias toward expansionary monetary policies, a bias that is muted by private sector influences on regional district bank appointments. The result is to confound the inflationary or deflationary contribution of central government versus regional district appointments, on the one hand, and political versus private sector appointments, on the other.

In comparison, the Bundesbank Council consists only of political appointees, and regional state representatives can outvote federal government appointees. The analysis of the German case has the potential to disentangle the role played by partially decentralized appointment powers relative to other political factors.

The political systems of the United States and Germany differ in another important respect, which makes intellectual arbitrage difficult.

[14] Nathaniel Beck, "Presidential Influence on the Federal Reserve in the 1970s," *American Journal of Political Science* 26 (August 1982); John T. Woolley, *Monetary Politics: The Federal Reserve and the Politics of Monetary Policy* (Cambridge: Cambridge University Press, 1984); Kevin B. Grier, "Congressional Influence on Monetary Policy: An Empirical Test," *Journal of Monetary Economics* 28 (October 1991); Henry W. Chappell, Thomas M. Havrilesky, and Rob Roy McGregor, "Partisan Monetary Policies: Presidential Influence through the Power of Appointment," *Quarterly Journal of Economics* 108 (February 1993); Thomas Havrilesky, *The Pressures on American Monetary Policy* (Dordrecht: Kluwer, 1995).

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Because the U.S. system is relatively more transparent, scholars have publicly available data to measure the monetary policy preferences of central bank appointees and the severity of conflict between the government and the central bank. Examples of such data are dissenting votes cast in FOMC meetings or the number of newspaper articles in which elected politicians and government officials complain about the direction of monetary policy.

By contrast, data about voting behavior on the Bundesbank Council are not publicly available. As a result, the link between political appointment powers and central bank accommodation can be tested only indirectly. Moreover, the federal government rarely pressures the Bundesbank in a publicly visible way, and only the most serious conflicts between the federal government and the bank are carried out in public. To the extent that political pressures and conflict between the government and the central bank are invisible to the scholar's eye, hypotheses about political influences on German monetary policy must rely on indirect measures. And since the use of indirect and possibly very crude measures has the potential to weaken the positive and normative conclusions drawn from the empirical analysis, it is all the more important to provide a careful motivation for the measures used.

III. A Theory of Political Agency

The electoral and party-political theories developed by Nordhaus, Hibbs, and Alesina are modified in two ways to permit their application to the institutions of German federalism and central banking: I examine the role of central bank independence and partially decentralized appointment powers. My analysis allows for two channels by which elected politicians can influence monetary policy—political pressures and the power of appointment.

The degree to which the central bank can set monetary policy independently of political pressures may vary continuously between zero and full independence. Political appointees may be partisans or technocrats. In accordance with the two theories developed by Nordhaus and Hibbs-Alesina, political appointees may be partisan in two ways. They may have a stake in the reelection chances of the federal government; or they may share the inflation preferences of their party's constituency.

My theory is general in the sense that it encompasses the hypotheses advanced by other scholars as special cases, obtained for some combination of assumptions about the degree of central bank independence

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(zero, full, or partial) and central bankers' types (partisans or technocrats). These two factors define the relationship between monetary growth, the timing of federal elections, and the partisan control of the federal government or central bank council (see Table 1 and Figures 2 and 3).

The extreme cases of zero and full central bank independence might appear to be straw men; the same holds for the simplistic assumption that central bankers are either partisans or technocrats. As a practical matter, there is little question that a political agency like a central bank enjoys some degree of discretion (derived at least in part from expertise or asymmetric information), but at the same time it cannot afford to be totally inattentive to the wishes of its political principals. Similarly, central bank appointees tend to be a mixed lot—perhaps because their political principals cannot fully predict or control the monetary policy preferences of an appointee or because the principals themselves have mixed motives in making the appointments. Nonetheless, for expositional purposes it is useful to study the implications of various extreme cases and simplifying assumptions and to clarify why leading scholars of German monetary policy arrive at different conclusions about the Bundesbank (they start out with different simplifying assumptions). As noted earlier, the political business cycle literature implicitly assumes zero independence when it treats the federal government and the Bundesbank as a unitary actor. Vaubel's assertion that the Bundesbank has both the capacity and (when controlled by the opposition) the incentives to obstruct the federal government's economic plans implicitly assumes full independence and partisan central bankers. Manfred Neumann views the Bundesbank as partially independent on some continuum between zero and full independence; he clearly has two channels of political control in mind when he calls for the Bundesbank's independence to be written into the German constitution ("institutional independence") and for a revision of the appointment procedures ("personal independence").[15]

The hypotheses that are obtained for various combinations of assumptions about the degree of central bank independence and central bankers' types are described in the following sections and further depicted in Table 1.

Zero Independence and Partisans or Technocrats (H_1)

Nordhaus's electoral politics hypothesis assumes that the federal government controls the money supply; the same holds for the Hibbs-

[15] Neumann, "Precommitment by Central Bank Independence," *Open Economies Review* 2 (1991).

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TABLE 1
MONETARY POLICY OUTCOMES AS A FUNCTION OF THE DEGREE OF CENTRAL BANK INDEPENDENCE AND CENTRAL BANKER TYPES

	Technocrats	*Partisans*
Zero independence	H_1 (zero independence and partisans or technocrats) —Monetary growth rates are higher prior to federal elections. —Monetary growth rates are lower (higher) if the federal government is controlled by a right-of-center (left-of-center) party.	
Partial independence	H_4 (partial independence and technocrats) —Monetary growth rates are set as a linear combination of the partisan monetary growth rate desired by the federal government and the non-partisan monetary growth rate desired by central bank technocrats, where the relative weight on the monetary growth rate preferred by the federal government decreases with the degree of central bank independence.	H_3 (partial independence and partisans) —Monetary growth rates are set as a linear combination of the partisan monetary growth rates desired by the federal government and by the median voter on the central bank council, where the relative weight on the monetary growth rate preferred by the federal government decreases with the degree of central bank independence.
Full independence	H_0 (full independence and technocrats) —Monetary growth rates are free of electoral or party-political effects.	H_2 (full independence and partisans) —Monetary growth rates are higher (lower) prior to federal elections if the federal government is supported (opposed) by a partisan majority on the central bank council. —Monetary growth rates are lower (higher) if the median voter on the central bank council was appointed by a right-of-center (left-of-center) party.

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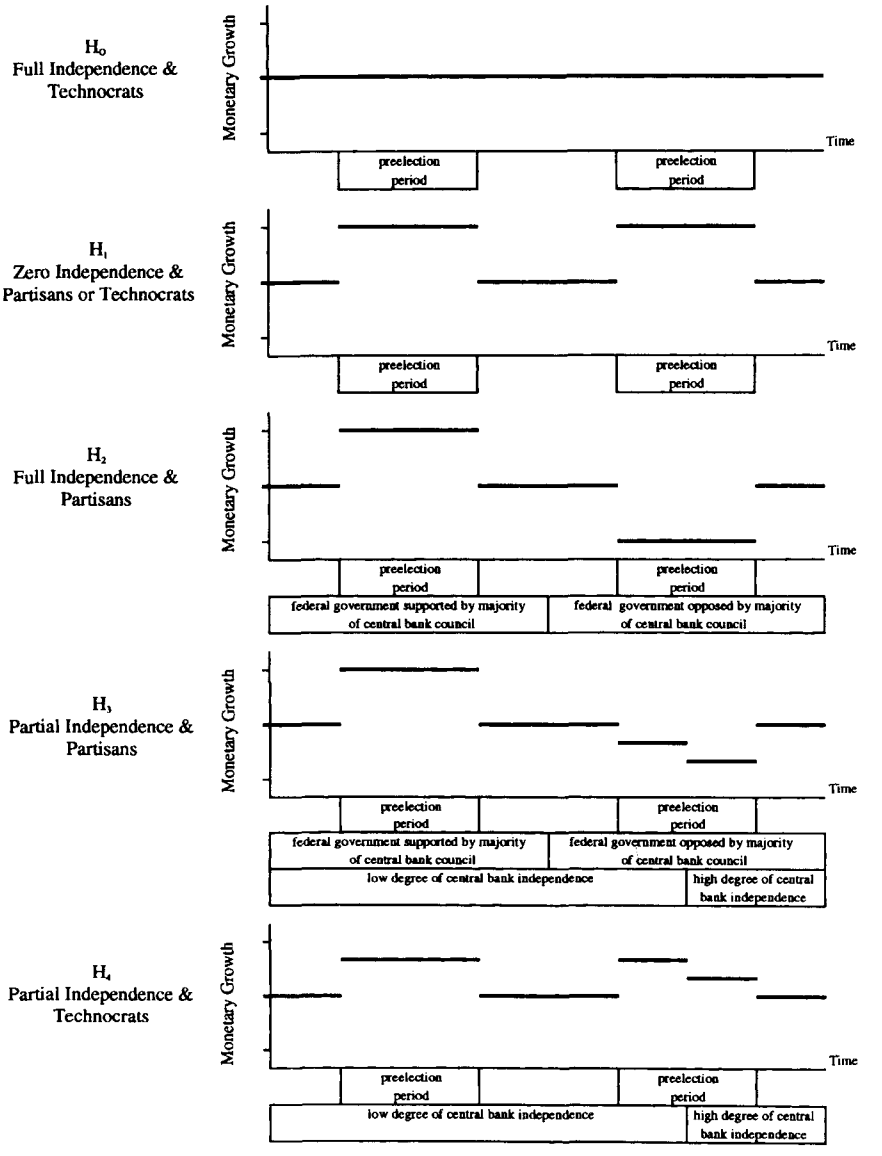

FIGURE 2
MONETARY GROWTH IMPLICATIONS OF THE ELECTORAL POLITICS MODEL

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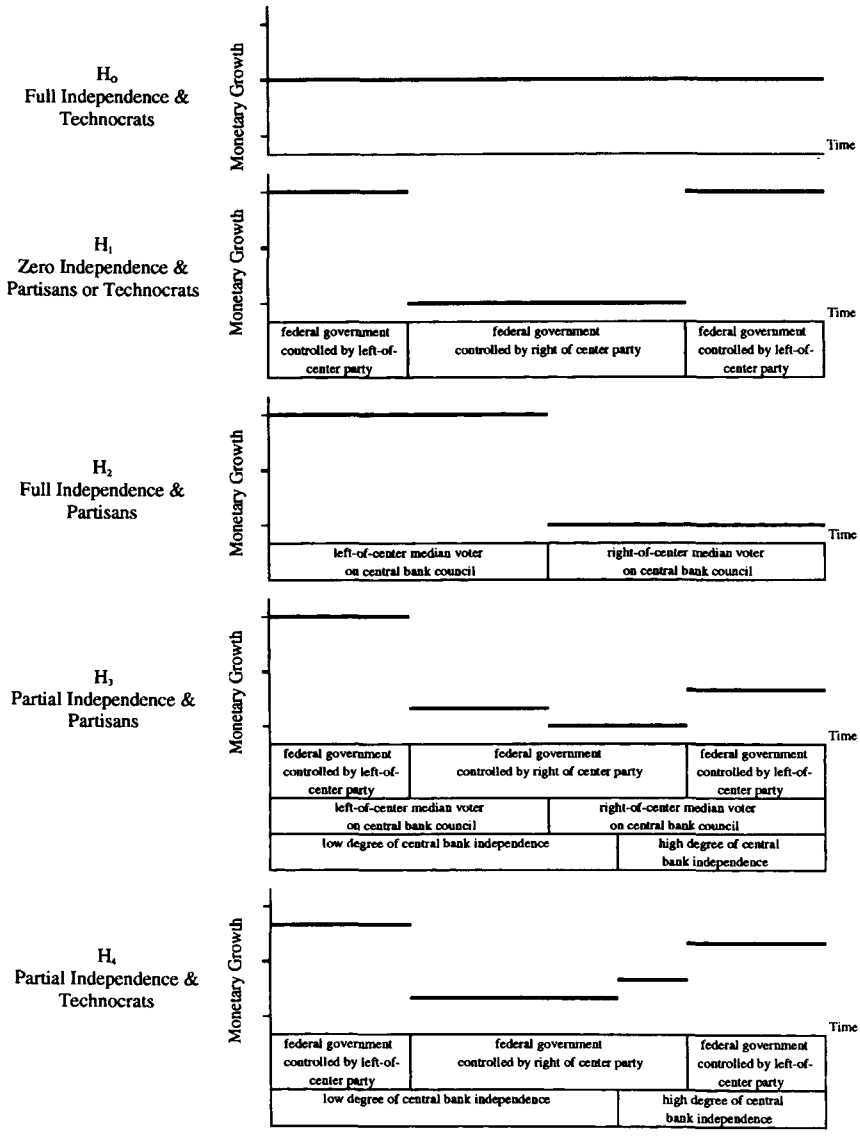

FIGURE 3
MONETARY GROWTH IMPLICATIONS OF THE PARTY POLITICS MODEL

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Alesina party politics hypothesis.[16] These two hypotheses obtain as special cases if the degree of central bank independence is zero, so that central bankers' types—partisans or technocrats—are irrelevant.

Full Independence and Partisans (H_2)

Next, I consider the case in which monetary policy is set by simple majority rule in a central bank council composed of a minority of federal government appointees and a majority of regional state appointees, all of whom share the electoral or party-political goals of their respective political principals.

Nordhaus's electoral politics hypothesis then turns into Vaubel's obstructionist hypothesis.[17] Suppose that both federal and regional policymakers prefer to have their party control the federal government. Their central bank appointees manipulate the money supply prior to federal elections. However, the sign of the preelection monetary stimulus depends on whether the party that is incumbent at the federal level controls the central bank council. If this is the case, then monetary growth will be higher in preelection periods—as before. But if the federal government faces a partisan opposition on the council, then monetary growth will be *lower* in preelection periods: the opposition-controlled central bank will attempt to thwart the reelection chances of the incumbent party by following a restrictive monetary policy prior to the election.

The Hibbs-Alesina party politics hypothesis is replaced by a median voter hypothesis: whether monetary growth is expansionary or restrictive is determined by the party affiliation of the median voter on the central bank council.[18]

[16] Nordhaus (fn. 3) formulated his hypothesis assuming adaptive inflation expectations and myopic voters. The Nordhaus hypothesis with rational inflation expectations and rational retrospective voting is developed by Torsten Persson and Guido Tabellini, *Macroeconomic Policy, Credibility and Politics* (Chur, Switzerland: Harwood Academic Publishers, 1990); Susanne Lohmann, "Rationalizing the Political Business Cycle: A Workhorse Model," *Economics and Politics* 10 (March 1998); idem, "Democracy, Inflation, and Growth: The Lucas Island Model Revisited" (Manuscript, UCLA, 1997); see also Kenneth Rogoff and Anne Sibert, "Elections and Macroeconomic Policy Cycles," *Review of Economic Studies* 55 (January 1988); and Kenneth Rogoff, "Equilibrium Political Budget Cycles," *American Economic Review* 80 (March 1990). Alesina (fn. 4) adds rational expectations to the Hibbs (fn. 4) model; see also Henry W. Chappell and William R. Keech, "Party Differences in Macroeconomic Policies and Outcomes," *American Economic Review, Papers and Proceedings* 76 (May 1986).

[17] The Vaubel hypothesis is formally developed in Susanne Lohmann, "Institutional Checks and Balances and the Political Control of the Money Supply," *Oxford Economic Papers* 50 (July 1998).

[18] The median voter hypothesis is formally developed in Susanne Lohmann, "Partisan Control of the Money Supply and Decentralized Appointment Powers," *European Journal of Political Economy* 13 (1997).

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The obstructionist and median voter hypotheses hold if the central bank is controlled by fully independent partisans.

Full Independence and Technocrats (H_0)

Maintaining the assumption that the central bank is fully independent, I now consider the possibility that central bankers are technocrats. In this situation monetary growth is not subject to electoral or party-political influences. This special case of my model—the central bank as benevolent dictator—serves as the null hypothesis.

Partial Independence and Partisans (H_3)

If central bankers are partially independent partisans, then the sign and size of the preelection monetary stimulus depend on the party control of the federal government and of the Bundesbank, with the degree of central bank independence determining the relative weight of the federal government and the Bundesbank; the same factors determine the degree to which monetary growth is expansionary or restrictive for party-political reasons.

Partial Independence and Technocrats (H_4)

If, instead, central bankers are partially independent technocrats, then electoral and party-political effects are increasingly muted as the degree of central bank independence rises.

IV. A Case Study of Institutional Change

The political impact of the Bundesrat veto point and its partisan control are now illustrated with a case study of the 1957 replacement of the Bank deutscher Länder by the Deutsche Bundesbank and the 1992 modification of the Bundesbank institution necessitated by German unification.[19] The case study serves two purposes. First, it motivates four measures of central bank independence utilized in the regression analysis. Second, it provides qualitative evidence complementing the quantitative results of the regression analysis.

After the Second World War the military occupation forces in West Germany—greatly influenced by the example of the Federal Reserve System—set up a decentralized system of Land central banks coordinated by their joint subsidiary, the Bank deuscher Länder in Frankfurt. The decision-making body of the system was composed of the presidents of the Land central banks and two further members of *their*

[19] For documentation of the primary and secondary sources I use, see Susanne Lohmann, "Designing a Central Bank in a Federal System: The Deutsche Bundesbank, 1957–1992," in Siklos (fn. 6).

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choice, the president of the central bank council and the president of the directorate. The Bank deutscher Länder was formally independent of the federal government.

The 1949 Constitution provided a mandate to replace military law with German law and establish a central bank. The political debate about the institutional design of the central banking system began soon thereafter and continued through two legislative periods: "No one directly called the independence of the future central bank into question. . . . At issue, instead, was the degree of centralization in the new banking system."[20]

The federal government initially favored a decentralized central banking system in which a majority of the members of the central bank council would be selected by the Land governments. However, when the Bank deutscher Länder followed a very restrictive monetary policy in 1955 and 1956, Christian Democratic chancellor Konrad Adenauer became unhappy with the bank's perceived failure to support economic reconstruction. Questioning the legitimacy of the bank's independence, the federal government subsequently developed legislation to implement a more centralized central banking system, in which the federal government would gain majority control of the central bank council via its appointment powers. The government's legislative proposal was supported by a majority in the Bundestag, but the Bundesrat expressed a preference for a decentralized system consistent with the "one Land, one Land central bank" principle.

Numerous proposals and counterproposals ended in compromise. The Bundesbank Law was passed by both houses of parliament in July 1957. The Bundestag vote was unanimous. In the Bundesrat, only two Länder (Bavaria and the Rhineland-Palatinate) voted against the law, and there is no obvious partisan pattern in the vote. (At the time Bavaria was governed by a coalition led by the Social Democrats, and the Rhineland-Palatinate by a coalition led by the Christian Democrats. Of the states that voted in favor of the Bundesbank Law in the Bundesrat, two were governed by coalitions with the Social Democrats as senior partner, three by coalitions with the Christian Democrats as senior partner, and four by Grand Coalitions.) Up to the end, there was considerable disagreement regarding the question whether the law was *zustimmungspflichtig* (requiring the approval of the Bundesrat). This issue remained unresolved when the compromise bill was passed by a two-thirds majority of the Bundesrat.

[20] John Goodman, "The Politics of Central Bank Independence," *Comparative Politics* 23 (April 1991), 337.

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The highly decentralized and independent Bank deutscher Länder was thus replaced by a partially decentralized and partially independent central banking institution. Until the law was modified in 1992, the council consisted of de jure up to ten and de facto between six and nine federal government appointees (the members of the directorate) and eleven regional state appointees (the Land central bank presidents). The formal independence of the Bundesbank is limited. The law stipulates that the bank is independent of instructions from the federal government in following its mandated goal of safeguarding the currency, but the Bundesbank is also required to support the general economic policy of the federal government. The law is ambiguous; the formulators of the Bundesbank Law deliberately chose not to specify how to resolve a potential conflict between the goal of safeguarding the currency and competing objectives of economic policy. The formulators of law considered the public airing of conflicts between the government and the central bank to be desirable, with the public (represented by the political parties and the media) serving as an umpire. The outcome of such a conflict would then depend on the public's relative trust for, respectively, its elected representatives and the appointed defenders of the currency. (Nowadays, the fact that the law requires the Bundesbank to support the economic policy of the federal government is generally interpreted as an empty gesture, but this interpretation is inconsistent with the legislative intent of the formulators of the Bundesbank Law. Bundesbank watchers appear to respond to the Bundesbank "success story" with the bias of hindsight: the degree of formal independence they read into the Bundesbank Law has increased over time.)

In the following decades, in fact, both the federal government and the Bundesbank would engage in a public dispute only when the stakes were high, given the considerable political costs associated with such a conflict. The government is aware that a public conflict can threaten its stability—especially if the popularity of the chancellor and his economic policies is ailing; in three cases, the Bundesbank directly or indirectly contributed to the resignation of a chancellor or to the collapse of a coalition government.[21] And for its part the Bundesbank knows that it can endanger the legitimacy of its independent status if it quarrels publicly with popular elected officials; in one case its resistance to the popular policies of a chancellor ended with the resignation of the Bundesbank president.[22] Even if a conflict is never actually fought out

[21] Ludwig Erhard in 1966, Kurt Georg Kiesinger in 1969, and Helmut Schmidt in 1982; see Marsh (fn. 13), 225.

[22] Karl Otto Pöhl in 1992; see Marsh (fn. 13), chap. 8.

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in public, awareness of the potential political costs of a public confrontation has an impact on monetary policy: it determines the degree to which the Bundesbank must accommodate political pressures in order to avoid a public debate.

In summary, the formal independence of the Bundesbank can be classified as only partial. Moreover, the Bundesbank's legal independence is complemented by the federalist components of its organizational structure. In the debate leading up to the passage of the Bundesbank Law, the decentralized structure of the central bank was celebrated as an institutional guarantee of its independence. Similarly, the pluralism of the appointing bodies and the participation of the Länder in the appointment process was thought to contribute to the bank's independence. Central bankers who are not subject to a unified political will were said to be more independent.[23]

The principle of central bank independence vis-à-vis the federal government was deemed crucial for safeguarding the currency: this principle would be institutionally guaranteed by the federalist components of the central bank's internal organizational structure, components that create legislative hurdles making future amendments to the Bundesbank Law difficult and time-consuming. Indeed, the 1957 law was said to be characterized by the "stamp of compromise" due to the "retarding influence of the Länder."[24] In the course of the debate, the "one Land, one Land central bank" principle was celebrated: a strong link between the federal structure of the Länder and the structure of the Bundesbank was associated with a high likelihood that the Länder would resist any attempts on the part of the federal government to change the Bundesbank Law or force the bank to do its bidding.

Several decades later, after German unification was completed on October 3, 1990, there occurred another vehement debate on the design of the German central banking system. The unification treaty of August 1990 required the Bundesbank Law to be modified within one year of unification, by October 3, 1991, with the objective of integrating the new East German Länder into the Bundesbank system. However, due to the fierce dispute between the federal and Land governments about the organizational form of the new central banking system, the deadline was not met: it took two years to modify the Bundesbank Law.

[23] Primary sources on the debate are collected in Deutsche Bundesbank, *Dreißig Jahre Deutsche Bundesbank: Die Entstehung des Bundesbankgesetzes vom 26. Juli 1957* (Thirty years Deutsche Bundesbank: The genesis of the Bundesbank Law of July 26, 1957) (Frankfurt am Main: Deutsche Bundesbank, 1988).

[24] Wilhelm Könneker, "Vom Zentralbanksystem zur Deutschen Bundesbank," *Zeitschrift für das gesamte Kreditwesen* 20 (1957), 796.

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As before, the key issue was the degree of centralization of the central banking system:

> German political elites, particularly in the ruling Christian Democrat–Free Democrat coalition and members of the Bundesbank Directorate . . . used the events of German unification and the discussions over European monetary integration as strategic rationalizations to hone the Bundesbank into a more centralized and less federal structure, a structure which will be . . . less dominated by [the presidents of the Land Central Banks].[25]

The federal government sought to increase the relative number of federal appointees on the Bundesbank Council by consolidating the Land central bank system. The government's bill was rejected by a two-thirds majority of the Bundesrat, which instead supported a proposal embodying the "one Land, one Land central bank" principle, while maintaining the maximum size of the directorate prescribed in the Bundesbank Law of 1957. Since the number of Länder had increased after German unification, this proposal amounted to a strengthening of the Land component on the central bank council.

The Bundestag passed the legislative proposal of the federal government by simple majority, largely along party lines with some abstentions. An overwhelming majority of the Bundesrat subsequently rejected that proposal and initiated the formal conciliation procedure. This majority contained the oppositional Social Democrats, who controlled a simple majority of the Bundesrat, as well as some Christian-Democratic Land governments. The latter were torn between representing the interests of their Länder, on the one hand, and supporting the proposal of the federal government, on the other.

The compromise proposal of the conciliation committee was subsequently rejected by a simple majority of the Bundesrat, which also declared the Bundesbank Law to be *zustimmungspflichtig*. The vote followed party lines, with the Social Democrats voting against the federal government's proposal. The initially close-to-united front of Länder broke up, as some Land representatives in the Bundesrat who had initially voted in favor of the Bundesrat's decentralized proposal switched their support to the federal government's bill. To undermine the opposition and convince wavering Christian Democrats in the Bundesrat, the federal government had modified its proposal. Indeed, it was not a coincidence that three states governed by the Social Democrats and only one Christian Democratic state were forced to give up

[25] Karl Christian Kaltenthaler, "The Bundesbank and German Federalism: The Politics of Institutional Change" (Manuscript, Washington University at St. Louis, 1992), 2–3.

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their Land central banks. To convince Christian Democrats to support its bill, the federal government had to make only marginal modifications to its proposal; in comparison, it would have been more expensive to make political side payments to get Social Democrats to vote against their party line. Overall, the consolidation and specific location of the Land central banks were determined not only by technocratic considerations but also by the logic of political side payments.[26]

If a two-thirds majority of the Bundesrat had voted against the proposal of the conciliation committee, or if changes to the Bundesbank Law had been classified consensually as *zustimmungspflichtig*, then the Bundestag would have had to counter the Bundesrat veto with a two-thirds majority—a majority that the federal government did not have in the Bundestag. In fact, partisan majorities in the two houses of parliament disagreed about the legal status of the Bundesbank Law. Thus, a simple majority of the Bundestag then proceeded to pass the changes to the law, based on the assumption that the bill did not require the approval of the Bundesrat. But the Bundesrat was operating on the opposite assumption. As a result, the government of the Rhineland-Palatinate, controlled by the oppositional Social Democrats, initially threatened to petition the Supreme Court for a finding that the law was *zustimmungspflichtig*, but it backed off when its Land central bank gained responsibility for the Saarland; the most vocal of the Social Democrats, Edgar Meister, was silenced with his appointment to the presidency of the Land central bank. (With the Banking Act that took effect on January 1, 1962, the Federal Banking Supervisory Office took on the task of regulating credit institutions in close cooperation with the Bundesbank. This change to the legal environment of the Bundesbank was not considered *zustimmungspflichtig*. Nonetheless, there was some uncertainty about how the Supreme Court would decide if approached in 1992, with a major change to the Bundesbank Law at stake.)

The Fourth Law Modifying the Bundesbank Law thus passed in June 1992. The law allows for nine Land central banks and up to eight members of the directorate. Some Länder are represented by their own Land central bank, while others share a Land central bank. The Bundesrat veto point thus ensured that Land appointees continue to constitute a majority on the council; but it failed to prevent a further dilution of the federalist components of the German central banking system. First, the ratio of regional state to federal government ap-

[26] Dietrich Dickertmann, "Deutsche Bundesbank: Landeszentralbanken vor einer Strukturreform?" (Deutsche Bundesbank: Land central banks facing a structural reform?) (Manuscript, University of Trier, 1991).

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pointees has decreased from an average of 11/8 in the 1957–92 period to an average of 9/8 in the 1993–97 period; the number of Land central bank presidents has declined absolutely, and when a Land central bank presidency remained vacant for some period, the number of Land central bank presidents was equal to the number of members of the directorate. Second, the "one Land, one Land central bank" principle has been given up. The Land governments that share a Land central bank must now agree on a compromise candidate for the presidency of their bank. That the consolidation of the Land central bank system undermines Land power was illustrated subsequently when the position of Land central bank presidency of the Länder Schleswig-Holstein, Hamburg, and Mecklenburg-West Pomerania remained vacant for a year because the Land governments—two of them controlled by the Social Democrats, the other one dominated by the Christian Democrats—could not agree on a candidate.

V. Regression Analysis

The purpose of the regression analysis is to move beyond anecdotal evidence suggesting that the behavioral independence of the Bundesbank fluctuated considerably during a time period when its legal status remained unchanged.[27] Although the Bundesbank institution did not change between 1957 and 1992, my empirical analysis employs quarterly data covering only the time period 1960–89. The data set begins with the year 1960 for two reasons. First, the Saarland and West Berlin are included in national income statistics only as of 1960. Second, a fair test of theories assigning an important role to political parties can be conducted only for a reasonably stable party system. In the 1950s, however, the West German party system was still very much in flux, whereas by the early 1960s, almost all special interest and regional parties had merged with other parties or dropped out of federal and regional state parliaments. The data set ends in 1989 to avoid confounding the monumental political changes associated with German unification in 1990 with other factors driving the Bundesbank's political responsiveness.

The time period 1960–89 has the potential to allow for a powerful test of the effects of divided government. It is characterized by several switches in power at the federal level. From 1960 to 1966, the right-of-center Christian Union parties (CDU/CSU) formed a government coalition with the centrist Free Democrats (FDP); a grand coalition of

[27] Rolf Caesar, *Der Handlungsspielraum von Notenbanken* (The discretionary leeway of central banks) (Baden-Baden: Nomos, 1981); Vaubel (fn. 6); Lohmann (fn. 19).

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CDU/CSU and the left-of-center Social Democrats (SPD) governed from 1966 to 1969; the SPD held power together with the FDP from 1969 to 1982, followed by a coalition of CDU/CSU and FDP. Both single-party and coalition governments occurred at the Land level. While a handful of Länder have been dominated by one party, no party has had a lock on a majority of the Land governments during this time period, and the party control of the Bundesrat has changed several times.

There is one potential drawback to testing the party politics hypothesis on data of this time period. A number of factors unrelated to German politics are associated with the worldwide inflation of the 1970s and disinflation of the 1980s, and these factors cannot be fully disentangled from the effects of left-of-center and right-of-center party control of the German government in the 1970s and 1980s, respectively.

One such factor is reflected in shifts in the academic (near) consensus among economists regarding the efficacy of Keynesian stabilization policies. The once-dominant doctrine of an inflation-unemployment tradeoff has been replaced by the widespread view that it is inappropriate to use monetary policy instruments to fight unemployment or raise output, as well as by a new recognition of the importance of price stability for aggregate welfare. The current doctrine provides the Bundesbank with an intellectual rationale to fend off political pressures for inflation as counterproductive and irresponsible.

Another factor is increasing international interdependencies and—of particular importance for monetary policy—financial integration. Sylvia Maxfield argues that the competition between countries for scarce capital intensifies with financial integration. To attract or reassure global investors and creditors, countries cede authority to central banks or reinforce existing authority, with disinflationary consequences.[28] It is worthwhile noting, however, that Germany has long been internationally very competitive with regard to the independence of its central bank.

The following are the regression variables.

— monetary growth rate$_t$: percentage growth rate of the seasonally adjusted central bank money stock in period t

— growth rate of gross national product$_t$: percentage growth rate of the gross national product in period t

— inflation rate$_t$: percentage growth rate of the consumer price index in period t

— rate of exchange rate depreciation$_t$: percentage rate of change of the exchange rate between the deutsche mark and the U.S. dollar in period t

[28] Maxfield, "Financial Incentives and Central Bank Authority in Industrializing Nations," *World Politics* 46 (July 1994).

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TABLE 2
PARTY CODES

Party Control of Federal and Land Governments[a]	*Party Code*
CDU/CSU or CDU or CSU	−2
CDU/CSU or CDU or CSU in coalition with FDP or other minor parties	−1
CDU/CSU or CDU or CSU in coalition with SPD and in some cases minor parties	0
SPD in coalition with FDP or other minor parties	1
SPD	2
SPD in coalition with GR or other "alternative" parties	3

[a]CDU/CSU: Christian Union Parties; CDU: Christian Democratic Union; CSU: Christian Social Union; FDP: Free Democratic Party; SPD: Social Democratic Party; GR: Green Party.

— Bretton Woods$_t$: dummy variable taking on the value one for periods t = 1960/1–1973/1 and the value zero for periods t = 1973/2–1989/4

— European Monetary System$_t$: dummy variable taking on the value zero for periods t = 1960/1–1979/1 and the value one for periods t = 1979/2–1989/4

— preelection period$_t$: dummy variable taking on the value one if period t is one of four quarters preceding a national election and the value zero otherwise

— party control of federal government$_t$: party code of the party coalition supporting the federal government in period t

— median voter on Bundesbank council$_t$: median party code of the members of the Bundesbank council in period t, where each member is assigned the party code of the federal or Land government that originally appointed him or her

— supportive Bundesbank majority$_t$: dummy variable taking on the value one in period t if the variable Median Voter on Bundesbank Council takes on the same sign as the variable Party Control of Federal Government and the value minus one otherwise

— Bundesrat support$_t$: number of Länder in period t for which the variable Party Control of Land Government takes on the same sign as the variable Party Control of Federal Government, where each Land is weighted by the number of its seats in the Bundesrat, except for West Berlin, which is assigned weight zero

— Länder support$_t$: number of Länder in period t for which the variable Party Control of Land Government takes on the same sign as the variable Party Control of Federal Government

— supportive Bundesrat majority$_t$: dummy variable taking on the value one in period t if the variable Median Voter on Bundesbank Council takes on the same sign as the variable Party Control of Federal Government and the value zero otherwise

— popularity$_t$: percentage of respondents to an Allensbach public-opinion poll who express support for the economic policies of the chancellor in period t

Table 2 presents coding for party control of federal and Land governments, and Table 3 contains the number of Bundesrat seats held by each Land. The descriptive statistics of the data are documented in Table 4; and the data themselves are available from the author upon request.

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TABLE 3
NUMBER OF BUNDESRAT SEATS HELD BY EACH LAND[a]

Land	*Number of Seats in Bundesrat*
Baden-Württemberg	5
Bavaria	5
Lower Saxony	5
North Rhine-Westphalia	5
Hesse	4
Rhineland-Palatinate	4
Schleswig-Holstein	4
Berlin[b]	3
Bremen	3
Hamburg	3
Saarland	3

[a] This table lists the number of Bundesrat seats held by each Land in the time period covered by the data set, 1960–89.

[b] Due to the special status of Berlin in the postwar period, the representatives of West Berlin in the Bundesrat had limited voting rights, and they could only participate in an advisory capacity in matters of central banking. For this reason, the Berlin delegates are assigned weight zero in the calculation of the variable Bundesrat Support.

The substantive significance of my empirical inquiry is supported by some intriguing patterns that appear in the descriptive statistics of German monetary growth. On average, the money supply expanded at a rate of 7.8 percent over the time period 1960–89. The average for the four quarters preceding each federal election is 8.2 percent. Monetary growth averages 7.8 percent under the Grand Coalition; 8.1 percent in periods of Social-Liberal rule; and 7.6 percent in periods of Christian-Liberal government (see Table 5). Taken individually, the differences between these means are not statistically significant, but the pattern as a whole is significant. Thus, electoral and party-political influences appear to be present in the raw data. I now conduct an Ordinary Least Squares (OLS) regression analysis to test the monetary policy implications of my theory.

FULL INDEPENDENCE AND TECHNOCRATS (H_0)

I first develop and test a base economic model that also serves as the null (benevolent dictator) hypothesis H_0, according to which German monetary policy is free of electoral and party-political influences.

The dependent variable is the growth rate of the seasonally adjusted central bank money stock. (All growth rates are calculated as fourth-quarter-to-fourth-quarter percentage growth rates.) The choice of monetary variable is guided by the fact that the Bundesbank targeted

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TABLE 4
DESCRIPTIVE STATISTICS OF VARIABLES, 1960/1–1989/4

Variable	*Mean*	*Median*	*Standard Deviation*	*Maximum*	*Minimum*
Economic variables					
monetary growth rate	7.813	7.916	2.272	13.5	3.197
growth rate of gross national product	3.118	3.303	2.573	9.487	−4.622
inflation rate	3.522	3.100	1.901	7.389	−.898
rate of exchange rate depreciation	−2.164	−.767	10.424	36.517	−27.914
Bretton Woods	.442	0	.499	1	0
European Monetary System	.358	1	.482	1	0
Political variables					
preelection period	.275	0	.448	1	0
party control of federal government	−.042	0	.956	1	−1
supportive Bundesbank majority	.217	1	.980	1	−1
median voter on Bundesbank council	−.003	0	.979	1	−1
Bundesrat support	23.975	25	7.153	41	15
Länder support	6.508	6	1.680	11	4
supportive Bundesrat majority	.408	0	.494	1	0
popularity	44.479	44.5	9.112	65	25

SOURCES: The economic data are taken from OECD, *Main Economic Indicators* (Paris: OECD, 1992). The popularity data were made available by the Institut für Demoskopie Allensbach. All other political variables were constructed by the author based on (1) the dates of federal and Land elections and the party control of federal and Land governments documented in *Zeitschrift für Parlamentsfragen* 3/1990, 470–72, and (2) the composition of the Bundesbank Council documented by the Bundesbank in its monthly and annual reports.

the central bank money stock from 1975 to 1987 (from 1988 onward, it targeted the monetary aggregate M3). As explained further below, I control for changes in the Bundesbank's reaction function across two regimes, the 1960–74 period, when the Bundesbank did not publicly commit itself to a monetary target, and the 1975–89 monetary targeting regime. The Bundesbank missed its targets seven out of fifteen times in the 1975–89 period, suggesting that the targets put weak constraints on monetary policy at best. For this reason, I do not control for the size of the targets.

Economic variables of concern to the Bundesbank are output growth, inflation, and external balance (the German economy is highly integrated into the world economy). I thus include the following inde-

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TABLE 5
AVERAGE MONETARY GROWTH RATES
UNDER VARIOUS POLITICAL REGIMES

Regime	*Average Monetary Growth Rate (%)*
All periods	7.8
Preelection periods	8.2
Period of grand coalition government	7.8
Periods of Christian-Liberal coalition government	7.6
Periods of Social-Liberal coalition government	8.1

SOURCES: See Table 4.

pendent variables in my regression equation: the one-quarter lagged growth rate of gross national product (GNP); the one-quarter lagged inflation rate of the consumer price index; and the one-quarter lagged exchange rate between the deutsche mark and the U.S. dollar.[29]

To capture the constraints on monetary policy implied by Germany's participation in the Bretton Woods fixed exchange rate system, I include a Bretton Woods dummy variable that takes on the value one in the time period 1960/1–1973/1 and the value zero otherwise. I allow for the possibility that the Bretton Woods regime constrains the Bundesbank's response to real and nominal variables by including as independent variables the product of the Bretton Woods dummy variable, on the one hand, and each of the economic controls (rates of GNP growth, inflation, and exchange rate depreciation), on the other. All of the Bretton Woods variables serve another function, namely, to control (approximately) for the possibility of a change in the monetary policy reaction function when the Bundesbank switched to a monetary targeting regime in December 1974.

Germany's subsequent membership in another fixed exchange rate system, the European Monetary System (EMS), is reflected in a second dummy variable that takes on the value one in the time period 1979/2–1989/4 and the value zero otherwise. I allow for the possibility that the EMS constrains the Bundesbank's response to real and nominal variables by including as independent variables the product of the EMS dummy variable, on the one hand, and each of the economic controls

[29] GNP is the standard measure of economic performance in German national income statistics; many other countries (including the United States) use gross domestic product instead. The exchange rate between the deutsche mark and the U.S. dollar is chosen because a significant amount of world trade is invoiced in dollars, and because the deutsche mark and the dollar are close substitutes in their role as international reserve currencies.

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(rates of GNP growth, inflation, and exchange rate depreciation), on the other.

The period not covered by the Bretton Woods and EMS dummy variables, 1973/2–1979/1, corresponds (approximately) to the period in which some member states of the European Community fixed exchange rate parities within prespecified margins of fluctuation, creating a "snake in the tunnel." Thus, the Bretton Woods and EMS dummy variables, both in their simple and multiplicative form, indirectly control for the monetary policy effects of this exchange rate regime—the snake. The dummy variables for the exchange rate regime also serve as indirect controls for the potentially inflationary effects of the oil price shocks that impinged on the German economy in the mid-1970s.

Preliminary regression results indicate persistence in the monetary growth rate over time. I add the one-period lagged monetary growth rate as a dependent variable.[30] The presence of a lagged dependent variable implies that one of the assumptions underlying standard OLS regression analysis is violated, namely, that the right-hand-side variables are independent of the disturbance term. As a consequence, the OLS estimators are biased. While the lagged dependent variable is correlated with past disturbances, it is contemporaneously independent of the disturbance term so that the OLS estimators are consistent. In a panel or cross-sectional study characterized by a low number of observations per unit of analysis, the bias may be unacceptably large. However, my time-series analysis is based on 115 observations.[31] The bias is consequently very small and does not affect the qualitative results of the analysis.[32]

Another problem associated with the presence of a lagged endogeneous variable is that the Durbin-Watson statistic may be biased. I deal with this problem by reporting the Durbin h statistic.

The regression results for the base economic model are reported in

[30] Here I follow Lang and Welzel (fn. 6).

[31] I use 115 observations and not the 120 that might be expected for quarterly data covering the time period 1960–89. This is because of the loss of the first five observations that occurs when lagged fourth-quarter-to-fourth-quarter growth rates are formed.

[32] Consider the model $y_t = \alpha + \beta y_{t-1} + \varepsilon_t$, where y is the dependent variable, α is a constant, β is the coefficient on the lagged dependent variable, and ε is a spherical disturbance. The bias of the OLS estimator of β, β^{OLS}, is given by $-\frac{1+3\beta}{T}$, where T is the number of observations. Note that the bias becomes small as T becomes large. (Moreover, the presence of additional regressors in the model decreases the bias.) One possible correction for the bias is to use the estimator $\frac{T\beta^{OLS}+1}{T-3}$; see Peter Kennedy, *A Guide to Econometrics*, 2d ed. (Cambridge: MIT Press, 1985), 122. The regression results summarized in Tables 6 and 7 show that my qualitative conclusions are unaffected by the bias, noting that T equals 115.

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Table 6. The constant and lagged monetary growth have significant explanatory power at the 1 percent level. The coefficients on the Bretton Woods and EMS dummy variables are negative in sign and are significant at the 1 percent and 5 percent levels, respectively. This result is consistent with our prior knowledge about the inflationary accommodation of the oil price shocks in the mid-1970s (the period not covered by the Bretton Woods and EMS dummy variables).

The coefficients on other economic controls—the rates of GNP growth, inflation, and exchange rate depreciation, both in their simple and multiplicative form—exhibit a more complex pattern of signs and levels of significance. The Bundesbank is primarily responsive to inflation. This result is consistent with the widespread view that the bank is single-minded in pursuit of its price-stabilization goal. Under the Bretton Woods regime, the coefficient on the inflation rate is positive and significant at the 1 percent level. In the post–Bretton Woods period inflationary pressures are met with a contraction in monetary growth; this effect is also significant at the 1 percent level. The EMS does not have an independent effect on inflation. These results are consistent with the conventional wisdom that the Bretton Woods regime significantly compromised the Bundesbank's pursuit of its price stabilization goal. The European snake is thought to have imposed little if any constraint on the conduct of Germany monetary policy. The insignificant impact of the EMS is consistent with the commonly held view that the deutsche mark is (de facto, if not de jure) the anchor currency of the system.

Admittedly, the assumptions that go into estimating a thirty-year Bundesbank reaction function are nothing short of heroic. I do not claim that mine is the last word on "how the Bundesbank conducts monetary policy."[33] Other scholars may allow for more sophisticated rules of "leaning against the wind" or take the constraints implied by the Bundesbank's monetary targets more seriously.[34]

Turning to the analysis of the political hypotheses, I find high levels of multicollinearity (and unsurprisingly so; see Figures 2 and 3). For this reason, I run a sequence of nonnested regressions, each of which

[33] Richard Clarida and Mark Gertler, "How the Bundesbank Conducts Monetary Policy," NBER Working Paper no. 5581 (May 1996).

[34] Ibid; and see also Ben Bernanke and Frederic Mishkin, "Central Bank Behavior and the Strategy of Monetary Policy: Observations from Six Industrialized Countries," in Olivier J. Blanchard and Stanley Fischer, eds., *NBER Macroeconomics Annual 1992* (Cambridge: MIT Press, 1992); Manfred J. M. Neumann and Jürgen von Hagen, "Germany," in Michelle U. Fratianni and Dominick Salvatore, eds., *Monetary Policy in Developed Economies: Handbook of Comparative Economic Policies*, vol. 3 (Westport, Conn.: Greenwood Press, 1993).

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TABLE 6
TESTING THE DEGREE OF CENTRAL BANK INDEPENDENCE AND CENTRAL BANKER TYPES[a]
(DEPENDENT VARIABLE: MONETARY GROWTH RATE, 1960/1–1989/4)

	Hypotheses				
Variables	H_0	H_1	H_2	H_3	H_4
Null model					
constant	4.627**	4.201**	4.523*	3.954**	4.339**
	(.904)	(.918)	(2.017)	(1.021)	(.893)
monetary growth rate$_{t-1}$	.723**	.741**	.719**	.740**	.739**
	(.060)	(.060)	(.061)	(.060)	(.059)
growth rate of gross	−.123	−.138	−.123	−.131	−.130
national product$_{t-1}$	(.079)	(.078)	(.117)	(.078)	(.077)
inflation rate$_{t-1}$	−.362**	−.371**	−.360	−.370**	−.376**
	(.125)	(.123)	(.262)	(.127)	(.122)
rate of exchange rate	.031	.016	.023	.010	.022
depreciation$_{t-1}$	(.023)	(.024)	(.032)	(.025)	(.023)
Bretton Woods	−3.409**	−2.933**	−3.353	−3.266**	−3.057**
	(.931)	(.980)	(2.592)	(1.033)	(.941)
Bretton Woods*growth rate	.222*	.205*	.202	.205*	.187*
of gross national product$_{t-1}$	(.095)	(.093)	(.127)	(.093)	(.093)
Bretton Woods*inflation	.619**	.585**	.652*	.645**	.601**
rate$_{t-1}$	(.183)	(.182)	(.302)	(.185)	(.180)
Bretton Woods*rate of	−.062	−.014	−.072	−.040	−.025
exchange rate depreciation$_{t-1}$	(.052)	(.059)	(.059)	(.055)	(.057)
European Monetary System	−2.061*	−1.365	−1.730	−1.424	−1.577
	(.886)	(1.022)	(1.463)	(1.057)	(.949)
European Monetary System*	−.095	−.095	−.123	−.116	−.092
growth rate of gross	(.119)	(.117)	(.145)	(.118)	(.117)
national product$_{t-1}$					
European Monetary System*	.201	.048	.141	.062	.100
inflation rate$_{t-1}$	(.159)	(.183)	(.275)	(.189)	(.167)
European Monetary System*	−.051	−.016	−.041	−.015	−.027
rate of exchange rate	(.027)	(.030)	(.035)	(.031)	(.028)
depreciation$_{t-1}$					
Electoral politics model					
preelection period		.578*			
		(.246)			
preelection period*			.301		
supportive Bundesbank			(.250)		
majority					
w*preelection period				.453*	
+(1-w)*preelection				(.197)	
period*supportive					
Bundesbank majority					

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TABLE 6 (*cont.*)

	Hypotheses				
Variables	H_0	H_1	H_2	H_3	H_4
preelection period* Bundesrat support					.022* (.009)
Party politics model					
party control of federal government		.144 (.173)			
median voter on Bundesbank council			.019 (.612)		
w*party control of federal government +$(1-w)$*median voter on Bundesbank council				.085 (.149)	
party control of federal government* Bundesrat support					.007 (.007)
Number of observations	115	115	115	115	115
R^2	.833	.842	.836	.842	.844
Adjusted R^2	.813	.820	.812	.820	.822
Durbin h-statistic	−.280	−1.261	−.567	−1.121	−.692
F(2, 102)-statistic		2.934	.765	2.879	3.384*

[a] Standard errors in parentheses; **p = .01; *p = .05; w=proportion of Bundesrat supporting federal government; the subscript $t-1$ indicates that the variable in question is lagged one period. The F-statistic assesses the marginal explanatory contribution of the hypothesis under consideration relative to the null hypothesis.

tests one hypothesis about the degree of central bank independence and central bankers' types against the null. The regression results are summarized in Table 6. The political variables are defined so that the predicted sign on their coefficients is positive.

ZERO INDEPENDENCE AND PARTISANS OR TECHNOCRATS (H_1)

To test the Nordhaus hypothesis, I create a preelection dummy variable that takes on the value one for four quarters prior to each federal election and the value zero otherwise.[35] The coefficient on this variable,

[35] This specification of the preelection dummy variable is standard in the empirical literature on political business cycles. Given that money affects employment and output with a lag of several quarters, this specification is not, strictly speaking, consistent with the idea that preelection monetary surges are meant to stimulate the economy and thereby increase the government's reelection prospects. I conjecture that increases in the money supply prior to elections reflect electorally motivated decreases in "visible" interest rates. This variant on the Nordhaus hypothesis is more plausible empirically, but it remains to be developed theoretically. For further discussion, see Lohmann (fn. 16, 1997).

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Preelection Period, has the right sign and is significant at the 5 percent level. On average, monetary growth increases by .578 percent in the four quarters preceding federal elections.

As a robustness check, I form an alternative preelection variable, taking into account that two federal elections were called early because the incumbent federal government had lost its majority in the Bundestag. To the extent that the early elections came as a surprise to the government and the Bundesbank (admittedly a questionable assumption), it is appropriate to set the preelection dummy variable equal to one only for those quarters of the preelection periods of the surprise elections that are part of the preelection periods of the originally scheduled federal elections. The regression results for this variable are similar but weaker: its coefficient has the right sign but is marginally insignificant at conventional levels (1 percent and 5 percent).

To test the Hibbs-Alesina hypothesis, I form a party code for the federal government (see Table 3 for details). The coefficient on the variable Party Control of Federal Government has the right sign but is insignificant at conventional levels. This result appears to suggest that the one-half percentage point difference in monetary growth rates across Christian-Liberal and Social-Liberal governments observed in the raw data does not reflect party-specific monetary policy preferences but rather reflects other economic or political factors. However, the insignificant impact of party-political factors may be an artifact of effective controls for economic factors. In particular, the dummy variables for the exchange rate regime, both in their simple and multiplicative form, distinguish three periods of moderate, high, and low inflation that might otherwise be attributed to the party control of the federal government. It exceeds the scope of this paper to examine how exchange rate regimes (with their implications for inflation) are chosen endogeneously, depending on the party in power.

Full Independence and Partisans (H_2)

To test Vaubel's obstructionist hypothesis, I create a dummy variable that takes on the value one if the median voter on the central bank council supports the federal government and the value minus one otherwise (see Table 3 for details). This variable is then multiplied with the preelection dummy variable. The coefficient on the resulting multiplicative variable, Preelection Period * Supportive Bundesbank Majority, has the right sign but is insignificant at conventional levels.

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Vaubel claims that his obstructionist hypothesis is consistent with the postwar data for a significantly large number of preelection periods, but he does not test his hypothesis in a regression analysis. I attempt to replicate Vaubel's result in a regression analysis that controls for a variety of economic variables. Since my partisan codes differ from Vaubel's, a failure to replicate his results could be due to coding differences. My partisan codes rely on publicly available information about the party composition of federal and Land governments that appointed the members of the Bundesbank Council. This coding procedure has the advantage of transparency and replicability. Vaubel, by contrast, relies on the assessment of five anonymous Bundesbank watchers (three academics and two bankers) to code the party affiliation of the members of the directorate. Since the partisan sympathies of individual members of the directorate do not always reflect the party composition of the federal government that originally appointed them, Vaubel's coding procedure has the potential to provide a more accurate picture of the party control of the Bundesbank Council. To ensure a fair test of the Vaubel hypothesis, I thus use an obstructionist dummy variable reflecting Vaubel's codes. The resulting variable does indeed perform better than the variable that is based on my coding procedure (as measured by t-statistics), but it is also insignificant at conventional levels.[36]

To test the median voter hypothesis, I assign to each member of the Bundesbank Council the party code of the federal or Land government that originally appointed him or her (see Table 2 for details). For each period, I then calculate the median value of the party codes of the members of the council. The coefficient for the variable Median Voter on Bundesbank Council has the right sign but is insignificant at conventional levels.

The median voter model is based on the assumption that the monetary growth preferences of Bundesbank appointees reflect the preferences of the principals who *originally* appointed them. A plausible alternative assumption is that the members of the central bank council vote the preferences of their *current* political principals. The latter assumption is appropriate if central bank appointees are concerned about reappointment. To investigate this alternative hypothesis, I form an alternative median voter variable based on the following coding procedure: for each quarter, the members of the council are coded according to the party affiliations of their current political principals in that quar-

[36] Berger and Woitek (fn. 6, 1997) also test and reject the Vaubel hypothesis.

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ter. It turns out that in the 1960–89 period the party control of the Bundesbank (as measured by this alternative median voter variable) *coincides* with the party control of the federal government. This result is not too surprising: between 1960 and 1989 the council was composed of eleven Land central bank presidents and between six and nine members of the directorate. In order to win majority control of the central bank (as measured by the alternative median voter variable), the opposition party would have had to gain power in at least nine or ten (of eleven) Länder—which never occurred.

According to the alternative median voter variable, the federal government was always supported by a partisan majority on the Bundesbank Council. Using this result, the regression results for the Nordhaus and Hibbs-Alesina hypotheses (H_1) can be reinterpreted. The hypothesis that the degree of central bank independence is zero so that monetary growth rates are determined by the party control of the federal government is *observationally equivalent* to the hypothesis that monetary growth rates are set by the median voter on the Bundesbank Council, assuming that the monetary policy preferences of central bank appointees are determined by their current political principals.

This result also sheds some light on the original median voter model, according to which central bankers' partisan sympathies are determined by the political principals who originally appointed them. The implications of the original median voter model, on the one hand, and those of the standard electoral and party politics models, on the other, differ primarily because of the eight-year terms of appointment enjoyed by members of the Bundesbank Council—appointments that are routinely renewed and staggered relative to the electoral terms of their political principals. The party control of the Bundesbank (as measured by the original median voter variable) does not generally coincide with the party control of the federal government due to the lags implied by the length and staggeredness of Bundesbank terms of appointment.

So far I have assumed that the decision-making process within the central bank council is adequately described by a median voter model. Given the importance of consensus in the rhetoric of central bankers, a plausible alternative hypothesis is given by a bargaining model according to which the monetary growth rate set by the central bank is an (equi-)weighted average of the monetary growth rates preferred by each member of the Bundesbank Council. As a robustness check, I replace the original median voter variable with the average party code across Bundesbank appointees. The results for the median and average

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voter variables are very similar: the coefficient on the average voter variable has the right sign but is also insignificant at conventional levels.

Partial Independence and Partisans (H_3)

To test whether the degree of central bank independence affects partisan monetary growth rates, I assume that the degree of independence decreases with the number of Bundesrat members who support the federal government. I utilize the Bundesrat measure of central bank independence because it is best motivated by the institutional analysis; the empirical performance of other measures will be discussed further below.

I form a linear combination of the preelection dummy variable, on the one hand, and the product of the preelection dummy variable and the dummy variable reflecting central bank support for the federal government, on the other. The relative weight of the first variable, w, is given by the proportion of Bundesrat members who support the federal government. The coefficient on the linear combination w * Preelection Period + (1 - w) * Preelection Period * Supportive Bundesbank Majority has the right sign and is significant at the 5 percent level.

Similarly, I form a linear combination of the party codes for the federal government and the median voter on the central bank council, where the relative weight of the former is given by the proportion of Bundesrat members who support the federal government, as before. The coefficient on the linear combination w * Party Control of Federal Government + (1- w) * Median Voter on Central Bank Council has the right sign but is insignificant at conventional levels.

Partial Independence and Technocrats (H_4)

To test whether the Bundesbank is controlled by partially independent technocrats, I multiply the preelection dummy variable by the Bundesrat measure of central bank independence. The coefficient on this multiplicative variable Preelection Period* Bundesrat Support has the right sign and is significant at the 5 percent level. The size of the coefficient reflects the substantive significance of this hypothesis: if the party coalition controlling the federal government holds (say) thirty-seven seats in the Bundesrat, monetary growth rates will be .484 percent [= (37 – 15) x .022%] higher on average than if the government enjoyed the support of only fifteen Bundesrat delegates.

The party code for the federal government is also multiplied by the Bundesrat measure of central bank independence. The coefficient on

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this multiplicative variable Party Control of Federal Government* Bundesrat Support has the right sign but is insignificant at conventional levels.

Based on the *F*-statistics reported in Table 6, I reject the null (benevolent dictator) hypothesis H_0 in favor of hypothesis H_4, whose marginal explanatory power is significant at the 5 percent level. (According to the *F*-statistics, none of the other political hypotheses outperforms the null at conventional levels of significance.) I conclude that the Bundesbank is staffed with nonpartisan technocrats who are partially insulated from political pressures. This result builds on the auxiliary hypothesis that the Bundesbank's independence decreases with partisan support for the federal government in the Bundesrat.

Maintaining the hypothesis that German monetary policy is set by partially independent technocrats, I now compare the empirical performance of the Bundesrat measure and other measures of central bank independence.

The measure Bundesrat Support implicitly attaches a greater weight to large Länder than to their smaller counterparts and zero weight to the city-state of West Berlin. In practice, the political leaders of small Länder can be very influential within their parties. Willy Brandt and Helmut Schmidt, for example, were mayors of the city-states of West Berlin and Hamburg, respectively, before they succeeded to the chancellorship; the leader of the small Saarland, Oscar Lafontaine, was the Social Democratic candidate for the chancellorship in the 1990 federal election. Thus, a second measure, Länder Support, is specified as the number of Land governments that support the federal government, thereby assigning equal weights to the Länder. A third measure, Supportive Bundesrat Majority, is binary, taking on the value one under unified party control of the Bundestag and the Bundesrat and the value zero under divided party control.

A fourth measure, Popularity, captures the notion that the degree of central bank independence decreases with the popularity of the federal government and its economic policies. It is equal to the percentage of public opinion poll respondents who express agreement with the economic policies of the chancellor.

These four measures are highly correlated, and unsurprisingly so (see Figure 4): if the federal government is unpopular, the parties forming the government coalition tend to lose votes in Land elections and consequently tend to lose control of Land governments and Bundesrat seats. For this reason, the second, third, and fourth measures of central bank independence serve as a robustness check for the Bundesrat mea-

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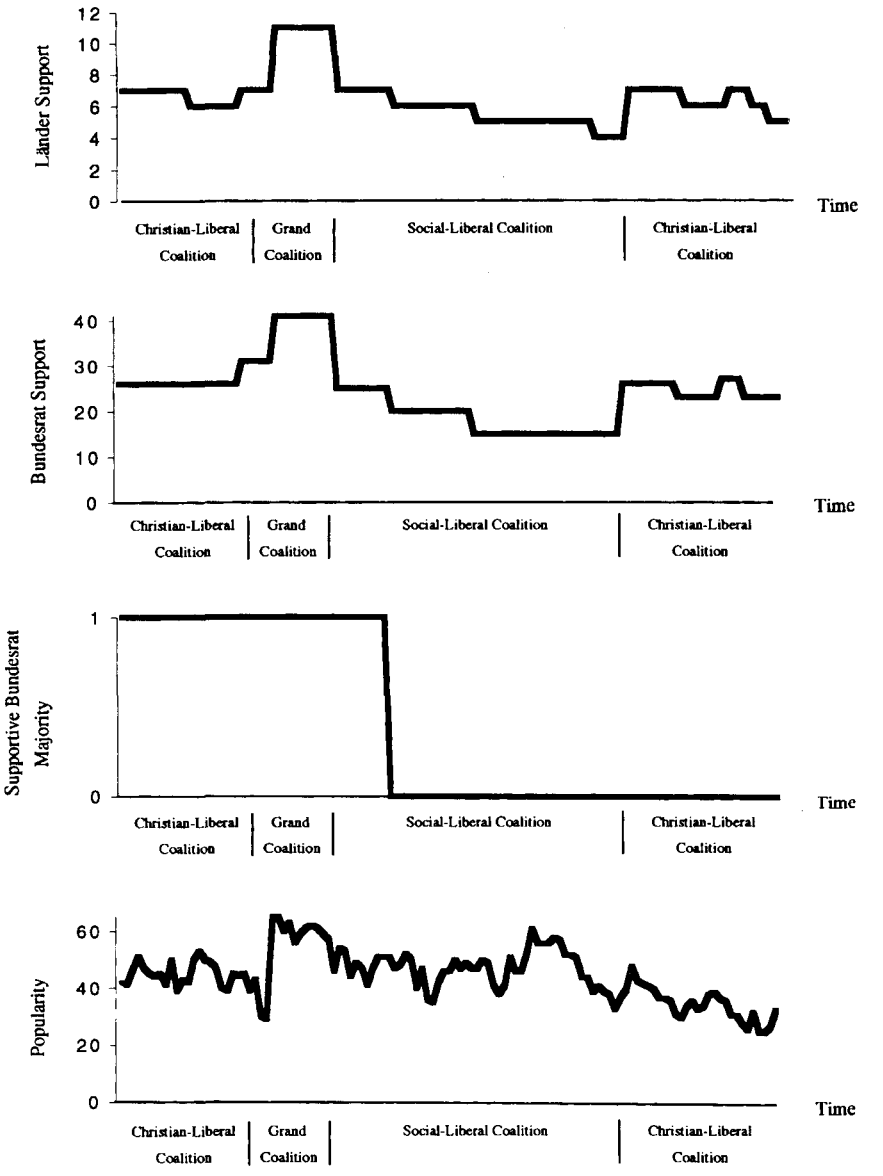

FIGURE 4
FOUR MEASURES OF CENTRAL BANK INDEPENDENCE

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TABLE 7
TESTING FOUR MEASURES OF CENTRAL BANK INDEPENDENCE[a]
(DEPENDENT VARIABLE: MONETARY GROWTH RATE, 1960/1–1989/4)

	Hypotheses			
Variables	H_4	H_4'	H_4''	H_4'''
Null model				
constant	4.339**	4.316**	4.638**	4.258**
	(.893)	(.900)	(.892)	(.924)
monetary growth rate$_{t-1}$	.739**	.738**	.721**	.736**
	(.059)	(.059)	(.060)	(.059)
growth rate of gross	−.130	−.132	−.122	−.135
national product$_{t-1}$	(.077)	(.077)	(.078)	(.078)
inflation rate$_{t-1}$	−.376**	−.374**	−.362**	−.367**
	(.122)	(.123)	(.123)	(.123)
rate of exchange rate	.022	.020	.031	.015
depreciation$_{t-1}$	(.023)	(.023)	(.023)	(.024)
Bretton Woods	−3.057**	−3.040**	−3.374**	−2.985**
	(.941)	(.953)	(.928)	(.995)
Bretton Woods*growth	.187*	.190*	.187	.200*
rate of gross national product$_{t-1}$	(.093)	(.093)	(.094)	(.093)
Bretton Woods*inflation	.601**	.595**	.613**	.586**
rate$_{t-1}$	(.180)	(.180)	(.181)	(.183)
Bretton Woods*rate of	-.025	−.026	−.058	−.020
exchange rate depreciation$_{t-1}$	(.057)	(.057)	(.057)	(.059)
European Monetary System	−1.577	−1.554	−2.063*	−1.407
	(.949)	(.973)	(.871)	(1.031)
European Monetary System*	−.092	−.096	−.095	−.115
growth rate of gross	(.117)	(.116)	(.117)	(.118)
national product$_{t-1}$				
European Monetary System*	.100	.096	.201	.059
inflation rate$_{t-1}$	(.167)	(.170)	(.157)	(.183)
European Monetary System*	−.027	−.024	−.051	−.018
rate of exchange rate	(.028)	(.028)	(.027)	(.030)
depreciation$_{t-1}$				
Electoral politics model				
preelection period*	.022*			
Bundesrat support	(.009)			
preelection period*		.084*		
Länder support		(.034)		
preelection period*			.787*	
supportive Bundesrat			(.347)	
majority				
preelection period*				.012*
popularity				(.005)

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TABLE 7 (*cont.*)

	Hypotheses			
Variables	H_4	H_4'	H_4''	H_4'''
Party politics model				
party control of federal government*Bundesrat support	.007 (.007)			
party control of federal government*Länder support		.020 (.027)		
party control of federal government*supportive Bundesrat majority		.121 (.209)		
party control of federal government*popularity				.002 (.004)
Number of observations	115	115	115	115
R^2	.844	.843	.842	.842
Adjusted R^2	.822	.821	.820	.820
Durbin h-statistic	−.692	−1.315	−1.051	−1.177
$F(2,102)$-statistic	3.384*	3.260*	2.848	2.932

[a] Standard errors in parentheses; $^{**}p = .01$; $^{*}p = .05$; the subscript $t-1$ indicates that the variable in question is lagged one period. The F-statistic assesses the marginal explanatory contribution of the hypothesis under consideration relative to the null hypothesis.

sure (the empirical performance of the popularity measure is of independent interest, as I will discuss later).

To deal with the multicollinearity problem, I again employ a nonnested approach. The regression results are reported in Table 7. The coefficients on all political variables have the right sign. Consistent with my earlier results, the variables representing the electoral politics hypothesis are significant at the 5 percent level, while the party-political variables are insignificant at conventional levels. The F-statistics reported in Table 7 indicate that the measure Bundesrat Support performs best, followed by the measures Länder Support, Popularity, and Supportive Bundesrat Majority. The hypotheses employing the first two measures outperform the null hypothesis at a 5 percent level of significance; the hypotheses utilizing the last two measures are marginally insignificant at conventional levels. These results suggest that the measures are robust. It is not too surprising that the Bundesrat and Länder measures do better than does the binary measure of unified versus divided government; the former measures are better motivated by the institutional analysis and make better use of the variability of the data.

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My hypothesis that popular governments have more influence over monetary policy than do unpopular governments is virtually the opposite of a proposition put forth by Bruno Frey and Friedrich Schneider.[37] Their model implies that unpopular governments have incentives to manipulate monetary policy instruments to increase their chances of reelection, while popular governments do not. Frey and Schneider thus focus on government popularity as a factor influencing the government's *desire* to manipulate the economy for electoral gain, while I emphasize the government's *ability* to do so.

In an attempt to replicate Frey and Schneider's empirical results, I modify the preelection dummy variable employed earlier, setting this variable equal to one in preelection periods only if the popularity of the chancellor's economic policies is below average and equal to zero otherwise. The coefficient on this variable has the right sign but is insignificant at conventional levels. In contrast, my regression results support my proposition about the negative implications of government popularity for central bank independence. However, I cannot exclude the possibility that the relatively weak empirical performance of my popularity measure is affected by the *offsetting* effects of government popularity identified by Frey and Schneider and myself.

Based on my regression results, I draw the following conclusions:

> — German monetary policy is subject to electoral pressures. There is no evidence that partisan preferences are influential via the power of appointment.
>
> — The Bundesbank Council is staffed with partially independent technocrats whose independence decreases with the partisan support for the federal government in the Bundesrat.

This conclusion synthesizes two positions: (1) that central bank appointees are depoliticized by *internal* peer group pressures within the Bundesbank and (2) that the Bundesbank is subject to *external* political pressures. Moreover, the auxiliary hypothesis that the Bundesrat veto point protects the Bundesbank's independence is the only hypothesis that is consistent with the evidence compiled in both the case study and the regression analysis.

Some qualifications apply. *First*, the regression coefficient on the preelection monetary surge is statistically significant at conventional levels, but by some commonsense measure its economic significance is not large. Here, it is useful to keep in perspective that my regression coefficients are comparable to the regression coefficients in the political business cycle literature or, more generally, in the literature on the po-

[37] Frey and Schneider (fn. 6); see also Kenneth A. Schultz, "The Politics of the Political Business Cycle," *British Journal of Political Science* 25 (January 1995).

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litical economy of government spending, budget deficits, and the like.[38] Inertia (the lagged dependent variable) tends to be the most important factor driving the dependent variable. By and large, the action (shocks triggering large monetary policy responses) lies in the economic fundamentals and not in political factors. Politics seems to matter only at the margin—keeping in mind that elections are often won and lost at the margin. The marginalization of politics is, of course, itself a political choice: in well-functioning political systems, well-designed institutions reduce the political vulnerability of economic policy.

Second, the insignificant impact of party-political effects may be due to the fact that they are easily confounded with a variety of economic factors driving inflation and disinflation in Germany, for which the regression analysis provides effective controls.

Third, because of the high degree of correlation between the institutional and popularity measures of central bank independence, it is reasonable to keep an open mind about the possibility that the Bundesrat veto does not matter. The institutional measures might capture the effect of government popularity on the government's capacity to influence monetary policy.

Fourth, the power of my statistical analysis is eroded by the necessity of having to control for a variety of economic factors. It is also undermined by the lack of publicly available data on voting behavior on the Bundesbank Council and of direct measures of government–central bank conflict. The subtle distinctions made by the theory—how various degrees of central bank independence and various central banker types translate into monetary policy outcomes—outrun the ability of the data to discrimate. Here, it is useful to keep in mind that theory has a purpose beyond generating testable hypotheses: it organizes our thinking about the amorphous mass of facts that constitute the real world.

VI. Discussion

Elected politicians delegate power to political agencies, but they impose procedural and institutional constraints to ensure the political control of the bureaucracy. In the United States such delegation regimes are shaped by divided versus unified party control of the presidency and Congress.[39]

[38] Alberto Alesina and Howard Rosenthal, *Partisan Politics, Divided Government, and the Economy* (New York: Cambridge University Press, 1995); James E. Alt and Robert C. Lowry, "Divided Government, Fiscal Institutions, and Budget Deficits: Evidence from the States," *American Political Science Review* 88 (December 1994); James M. Poterba, "State Responses to Fiscal Crises: The Effects of Budgetary Institutions and Politics," *Journal of Political Economy* 102 (August 1994).

[39] Mathew D. McCubbins, Roger G. Noll, and Barry R. Weingast, "Administrative Procedures as Instruments of Political Control," *Journal of Law, Economics, and Organization* 3 (Fall 1987); David

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This article analyzes the political control of a specific agency (the central bank) in a parliamentary system characterized by strong party discipline and separation of powers between federal and regional state governments.

There is an important distinction between the logic underlying the political control of the bureaucracy in general and of central banks in particular. The new institutionalism asserts that political principals have ex ante incentives to structure the environment of political agencies with the objective of *preventing* the bureaucracy from developing discretionary powers, thereby ensuring its ex post political responsiveness. In contrast, elected politicians have ex ante incentives to *depoliticize* monetary policy decisions by (at least partially) insulating central bankers from ex post political pressures. One reason is that rational inflation expectations undermine the potential of monetary policy to produce systematic real effects on the economy that might further the electoral fortunes of incumbent politicians. Another reason is that politicians anticipate that they might be in the opposition in the future. As a result, they may be better off ex ante ensuring that monetary policy is not vulnerable to political pressures of any kind.

My analysis counters two prevalent views in the literature on central banking institutions and credible commitment. One view is that legal statutes, or formal institutions more generally, have the capacity to solve commitment problems; that is, the mere passing of a law creates commitment where none existed before. The opposite view is that "institutions do not matter";[40] that is, they are merely epiphenomenal to the underlying political interests and have no independent policy impact.

The first view is implicit in the statute-reading methodology scholars employ when they infer causality from the negative cross-country correlation between legal central bank independence and low inflation.[41] It is also present when scholars propose that the political vulnerability of monetary policy can be effectively reduced by the simple means of writing central bank independence into the constitution or making changes to the independent status of the central bank subject to a two-thirds majority in parliament. This view is inconsistent with

Epstein and Sharyn O'Halloran, *Delegating Powers: A Transaction Cost Politics Approach to Policy Making* (New York: Cambridge University Press, 1998); Susanne Lohmann and Sharyn O'Halloran, "Divided Government and U.S. Trade Policy," *International Organization* 49 (Autumn 1994).

[40] Susanne Lohmann, "Why Do Central Banking Institutions Matter?" (Paper presented at the annual meeting of the American Political Science Association, San Francisco, September 1996).

[41] James Forder, "On the Assessment and Implementation of 'Institutional' Remedies," *Oxford Economic Papers* 48 (January 1996), 39.

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time-series and cross-country evidence suggesting that formal independence does not map into behavioral independence in a straightforward way. For example, the Bundesbank's formal status remained unchanged from 1957 to 1992. Yet the degree to which German monetary policy was vulnerable to political pressures fluctuated considerably over this time period. Similarly, we find that measures of legal central bank independence are negatively correlated with the depreciation in the real value of money for *developed* countries, whereas the correlation is effectively zero for *developing* countries: "The divergence between the letter of the law and actual practice is substantially higher in developing than in developed countries. This may be due to a general norm of more adherence to the law in developed countries."[42] To understand the constraining role of legal statutes, or formal institutions more generally, there is no serious alternative to examining the way they are embedded in a larger political system that creates political costs of violating or changing the law and political costs of institutional corruption, change, or breakdown. In short, "reading statutes does not measure independence, passing them does not create it."[43]

The second view holds that legal statutes, or formal institutions, cannot magically solve problems of credible commitment.[44] If politicians cannot credibly commit themselves to a sound monetary policy (however defined), there is good reason to believe that they cannot credibly commit themselves to respect the independence of a central bank that would follow such a policy if left to its own devices. After all, politicians who have the power to set up an institution in the first place also have the power to violate the institution at their discretion. When politicians make policy promises in institutionalized form, the problem of credible commitment simply shifts to another level—instead of defecting on a policy promise, politicians may defect on the institutional promise.

It follows from this argument that a formally independent central bank will be able to follow a sound monetary policy only if elected politicians (and ultimately voters) want such a policy; or if there exists a coalition of inflation-averse interests politically capable of protecting

[42] Alex Cukierman, *Central Bank Strategy, Credibility, and Independence: Theory and Evidence* (Cambridge: MIT Press, 1992), 421.

[43] Forder (fn. 41), 50.

[44] Bennett T. McCallum, "Two Fallacies Concerning Central-Bank Independence," *American Economic Review Papers and Proceedings* 85 (May 1995); Lohmann (fn. 40); idem, "Reputational versus Institutional Solutions to the Time-Consistency Problem in Monetary Policy," in Sylvester C. W. Eijffinger and Harry Huizinga, eds., *Positive Political Economy: Theory and Evidence* (New York: Cambridge University Press, 1997).

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the integrity of the institution against inflation-prone politicians.[45] Thus, we might explain low inflation in Germany with reference to the German people's aversion to inflation, fed by their historical experience with hyperinflation. Or we might invoke German political culture—specifically the widespread distrust of politicians and political parties—to explain why changes to the Bundesbank's independent status are taboo in German political discourse.[46] Alternatively, we might conceive of the Bundesbank institution as being protected, or captured, by inflation-averse private banking interests.[47]

I argue that monetary institutions allow for credible commitment in situations where informal policy promises are incredible. They are not simply derivative of the underlying political interests: the mapping of interests onto monetary policy outcomes, as it is shaped by monetary institutions, is not straightforward. The institutional membrane that shapes this mapping is, of course, at some point in time the object of political choice. In this sense, monetary institutions are determined at the institutional design stage by the policy preferences of the relevant political actors, who anticipate (up to a point) how various institutional arrangements translate into policy outcomes at the policy-making stage.[48] Once a central banking institution is set up, however, making any changes to the institution is difficult or costly due to the presence of "political veto players."[49] Indeed, a central point of my analysis is that elected politicians deliberately set up veto points in order to insulate monetary policy from counterproductive political pressures. The stickiness of institutional arrangements implies that the relevant political actors, their preferences, or other factors determining their relative power may change while the institution remains constant. In the case under consideration, the federal components of the Bundesbank institution are designed to give the Länder a stake in the institution, thereby turning the Bundesrat into an important veto player; the impact of this veto point varies over time as a function of divided versus unified party control of the federal and regional state governments.

[45] Adam Posen, "Why Central Bank Independence Does Not Cause Low Inflation: There Is No Institutional Fix for Politics," in Richard O'Brien, ed., *Finance and the International Economy*, vol. 7 (Oxford: Oxford University Press, 1993); idem, "Declarations Are Not Enough: Financial Sector Sources of Central Bank Independence," in Ben Bernanke and Julio Rotemberg, eds., *NBER Macroeconomics Annual 1995* (Cambridge: MIT Press, 1995).

[46] Caesar (fn. 27).

[47] Posen (fn. 45, 1993, 1995); see also C. Randall Henning, *Currencies and Politics in the United States, Germany, and Japan* (Washington, D.C.: Institute for International Economics, 1994).

[48] Lohmann (fn. 2).

[49] George Tsebelis, "Decision Making in Political Systems: Veto Players in Presidentialism, Parliamentarism, Multicameralism, and Multipartyism," *British Journal of Political Science* 25 (July 1995).

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There exists some evidence that my hypothesis about federalist checks and balances and central bank independence generalizes to other countries. King Banaian, Leroy Laney, and Thomas Willett find a positive cross-country correlation between federalism and central bank independence.[50] In a more subtle test, Peter Moser separates the OECD countries into three groups. The first group has strong checks and balances: these countries have at least two legislative bodies with different preferences, both of which have veto powers. (Moser infers differences in preferences from differences in the regional allocation of seats, electoral rules, terms of office, or party control.) The second group has weak checks and balances: one of the legislative bodies does not have veto powers but has only the capacity to delay. The third group has no checks and balances: these countries have only one legislative body, or if they have more than one, the two bodies have identical preferences. Moser's regression analysis shows that the negative correlation between legal central bank independence and inflation is strongest for countries with strong checks and balances and weakest for countries with no checks and balances. (Moser obtains this result after controlling for the fact that central banks in countries with stronger checks and balances tend to enjoy more legal independence.)[51]

As intriguing as this evidence is, any sweeping conclusions about the link between federalism and central bank independence are suspect. Part of my message is that the specifics of how a central bank is embedded in a larger political system matter a great deal, and for this reason my analysis is unlikely to generalize to other countries in a straightforward way. In developed countries, the *most independent* central banks are found in federal systems: Germany, Switzerland, the United States, and Canada. But in developing countries, at least until the recent spate of reforms, some of the *least independent* central banks have been found in federal systems: Brazil, Argentina, Venezuela, and Mexico. In many developing countries, a considerable proportion of government spending is financed by inflation. Governments facing electoral or party-political pressures to provide public goods, subsidize state enterprises, and redistribute income may prefer a compliant, finance-providing central bank. In such settings, it is plausible that the

[50] Banaian, Laney, and Willett, "Central Bank Independence: An International Comparison," in Eugenia Froedge Toma and Mark Toma, eds., *Central Bankers, Bureaucratic Incentives, and Monetary Policy* (Boston: Academic Publishers, 1986).

[51] Peter Moser, "Checks and Balances and the Supply of Central Bank Independence," *European Economic Review* (forthcoming).

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number of veto players (or the strength of regional states in a federal system) is negatively correlated with central bank independence.[52] A systematic comparative analysis of the relationship between federalism and central bank independence clearly remains an important topic for future research.

[52] Sylvia Maxfield, *Gatekeepers of Growth: The International Political Economy of Central Banking in Developing Countries* (Princeton: Princeton University Press, 1997).

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MOBILIZATION, SOCIAL NETWORKS, AND TURNOUT
Evidence from Japan

By GARY W. COX, FRANCES M. ROSENBLUTH, and MICHAEL F. THIES*

Recent work explaining voter turnout in the United States has focused on the strategic role of parties, unions, and other elite actors in mobilizing voters. In fact, there are two strands of literature that begin with the notion of strategic elites but then proceed along different paths. The first strand goes back at least to V. O. Key and argues that elites exert more mobilizational effort when elections are closer (because they believe that their efforts are more likely to make the difference between winning and losing).[1] Greater elite efforts in turn boost turnout, which explains why closeness and turnout are positively correlated.[2] This strand addresses the electoral circumstances that encourage elites to mobilize.

The second strand, in contrast, focuses on *whom* elites seek to mobilize.[3] Part of the answer is straightforward: a party will target those unlikely to vote if not mobilized, but who would very likely support the party in question if they did get to the polls. Another, less obvious part of the answer is that rational mobilizers should target supporters plugged into wider and more tightly knit social networks, in the hope of producing favorable secondary mobilization. This incentive of mobilizing elites to focus on socially connected supporters may partly explain both some well-known microcorrelations between individual characteristics (for example, home ownership) and turnout and some well-known macrocorrelations between aggregate characteristics (for example, urbanness) and turnout.[4]

* We thank Michael Munger and Steve Reed for helpful comments; Ikuo Kabashima for sharing his census data with us; and Ross Masahiko Asano, Mitsue Sakamoto, and Robin Moriyama for research assistance. Michael Thies thanks the UCLA Academic Senate Committee on Research for its support.

[1] Key, *Southern Politics in State and Nation* (New York: Knopf, 1949).

[2] For a review of research that finds a closeness-turnout correlation, see John Matsusaka and Filip Palda, "The Downsian Voter Meets the Ecological Fallacy," *Public Choice* 77 (December 1993).

[3] Steven J. Rosenstone and John Mark Hansen, *Mobilization, Participation, and Democracy in America* (New York: Macmillan, 1993).

[4] Ibid.

World Politics 50 (April 1998), 447–74

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Although both of these strands in the literature begin with the common premise that political elites are strategic and conclude that elite efforts affect electoral participation in important ways, to date they have been dealt with quite separately. In this paper, we put the two parts of the model together. Such an approach is logically necessary because whether a given effort is worth the cost depends both on how many votes that effort will garner (the effort's *vote yield*) and on whether those additional votes are sufficient to alter the outcome (closeness). Elites who have very poor targets of mobilization and cannot produce enough votes to affect even a close election should not expend effort. Similarly, even elites with excellent targets of mobilization should not bother to contact them if the additional votes will not affect the outcome. Thus, if one believes that social capital is a key factor in the vote productivity of elite mobilizational efforts, then *the level of elite effort (hence turnout) should be an interactive function of closeness and social capital.*

In order to test our hypothesis, we extend the range of application of the strategic elites model from the single-member plurality systems to which it has hitherto been confined (mostly in the U.S., the U.K., and Canada) to the multimember single nontransferable vote (SNTV) system formerly used in Japan. Japan is an interesting case for two reasons. First, the literature on Japanese turnout clearly emphasizes the importance of candidates' efforts to mobilize their supporters, especially by activating key social networks in the district, yet there has been no analysis of the interaction of closeness and social capital. Second, Japan's medium-size districts, used during the period we study here, occupy an interesting theoretical position. Amy and Powell contend that elections in large multimember districts operating under proportional representation (PR) are almost always competitive, so that elites almost always mobilize—which partly explains why turnout rates under PR are generally high.[5] Were Japan's mostly three-to-five seat districts large enough for a similar result to play out? Or were they still small enough to produce significant responses at the elite and mass level to variations in closeness?

The rest of the paper proceeds as follows. We begin by reviewing and extending the strategic elites model. We then set the stage for adapting the model to Japan by reviewing the literatures on mobilization and social networks in Japanese politics. Next we derive hypotheses about the determinants of elite mobilizational efforts in Japan and test them. Our

[5] Douglas J. Amy, *Real Choices/New Voices: The Case for Proportional Representation Elections in the United States* (New York: Columbia University Press, 1993); G. Bingham Powell Jr., "American Voter Turnout in Comparative Perspective," *American Political Science Review* 80 (March 1986).

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results show that election closeness and turnout are positively correlated in elections to the Japanese Diet (as elsewhere), that this correlation is mediated by elite behavior (as elsewhere), and that turnout varies with the product of election closeness and the social density of the electoral district (something that has yet to be investigated elsewhere).

The Strategic Elites Model

Closeness and Mobilizational Effort

Developed by Key and recently reexamined by Morton, Uhlaner, Cox and Munger, Matsusaka, Rosenstone and Hansen, and Nalebuff and Shachar, the first branch of the strategic elites model focuses on how closeness affects mobilizational effort.[6] Because elite efforts are more likely to be decisive in such elections, the model predicts greater elite effort in close elections. Because elite efforts are presumed to be effective in producing votes, the model also predicts greater turnout in close elections.

There is good evidence for U.S. elections that both of these postulated causal steps operate in practice. First, Cox and Munger show that expenditures by candidates for the U.S. House of Representatives increase with closeness, while Rosenstone and Hansen and Nalebuff and Shachar show that the number of voters contacted by party workers increases with closeness.[7] So elites do seem to exert more effort (whether measured by expenditures or contacts) in close races. Second, Cox and Munger show that total expenditures in U.S. House elections correlate positively with turnout (controlling for a battery of other variables), while Rosenstone and Hansen review a large literature that shows the efficacy of party contacts in boosting turnout in the U.S. and the U.K. Parties and candidates do seem able to boost turnout via greater mobilizational efforts.

Despite the empirical success of these simple predictions from the strategic elites model, it is not the only model that predicts a correla-

[6] Key (fn. 1); Rebecca Morton, "A Group Voting Model of Public Good Provision," *Social Choice and Welfare* 4 (May 1987); idem, "Groups in Rational Turnout Models," *American Journal of Political Science* 35 (August 1991); Carole Uhlaner, "Rational Turnout: The Neglected Role of Groups," *American Journal of Political Science* 33 (May 1989); Gary W. Cox and Michael Munger, "Closeness, Expenditures and Turnout: The 1982 U.S. House Elections," *American Political Science Review* 83 (March 1989); John Matsusaka, "Election Closeness and Voter Turnout: Evidence from California Ballot Propositions," *Public Choice* 76 (December 1993); Rosenstone and Hansen (fn. 3); Barry Nalebuff and Ron Shachar, "Follow the Leader: Theory and Evidence on Political Participation" (Manuscript, Yale University, 1995).

[7] Rosenstone and Hansen (fn. 3), 164, 169–70.

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tion between closeness and turnout; Riker and Ordeshook's well-known pivotal voter model does so as well.[8] More important, the strategic elites model suffers from some internal ambiguities. We shall consider two such ambiguities here.

First, in close races elites have options other than mobilizing: they can, for example, increase their persuasive efforts or try to demobilize their opponents' supporters. Thus, the expectation that closeness boosts mobilizational effort hinges on a tacit assumption that elites' best response to closeness is more mobilizational than demobilizational.[9] In the case of Japan, a presumption in favor of mobilization seems amply justified (see below) and so we focus here on a second problem, namely, that closeness is an incomplete proxy for the probability of elite mobilizational effort being decisive. In order to explain this point, we turn to the other branch of research into the strategic elites model, that concerning *whom* elites will mobilize.

The Targeting of Elite Mobilizational Effort

Rosenstone and Hansen note that one way for rational elites to get the most out of their mobilizational effort is to target voters who will respond not just by voting but also by urging their friends, families, and acquaintances to vote as well.[10] As these secondary contacts will be more numerous when directly contacted individuals have more friends, families, and acquaintances to urge, Rosenstone and Hansen predict that elites will focus their mobilizational efforts on those embedded in denser social networks. To support this prediction, they show that people who have stronger community ties due to long residence, home ownership, and church attendance are considerably more likely to be contacted.[11] They also show that contacted voters are more likely to urge others to vote.[12] Thus, for the U.S., Rosenstone and Hansen show both that political parties target their mobilizational efforts at those more centrally located in social networks and that the rationale for

[8] William Riker and Peter Ordeshook, "A Theory of the Calculus of Voting," *American Political Science Review* 62 (March 1968). Of course, doubts about the theoretical standing and empirical success of the pivotal voter have been expressed by, for example, Cox and Munger (fn. 6); Matsusaka (fn. 6); and Robert A. Jackson, "The Mobilization of Congressional Electorates," *Legislative Studies Quarterly* 21 (August 1996).

[9] Elites might engage in both mobilization and demobilization (and persuasion). As long as the total turnout boost from mobilization exceeds the total turnout decline from demobilization (and perhaps from persuasion via negative advertising), turnout and closeness will covary positively, ceteris paribus.

[10] Rosenstone and Hansen (fn. 3).

[11] Ibid., 164–65.

[12] Ibid., 171.

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doing so is borne out—those contacted do engage in secondary mobilization, in addition to voting themselves.

A Unified Model

In this paper, we extend the strategic elites model by considering how elite decisions to mobilize depend on the interaction between closeness and social density.[13] The main point is that even a purely instrumental view of elite decisions—in which they mobilize only if the probability of their effort deciding the election (p), times the benefit of doing so (B), exceeds the cost of mobilization (c)—may entail that social density is just as relevant as closeness to explaining levels of mobilization and turnout. This is because the probability of an elite actor's effort deciding the outcome of an election depends *both* on how many votes they need to determine the outcome (closeness) *and* on how many votes their effort is likely to garner (vote yield). If the vote yield of mobilizational effort depends on the social capital of those contacted, then the p term in the elite calculus of effort will depend interactively on closeness and social density.

Should the vote yield of a given mobilizational effort depend substantially on the social capital of those contacted? Let Y(k) be the number of new votes yielded when a campaign contacts k people. To simplify things, assume that the campaign has correctly identified its supporters: if they turn out, they will support the candidate on whose behalf the contacts are made. In this case, the number of new votes that contacting k more people yields depends not just on how much more likely those contacted are to vote but also on how many secondary contacts each initial contact makes (on average) and on how many of these secondary contacts turn out (but would not have otherwise). If those contacted had an average probability t of voting prior to being contacted, and an average probability $t + \delta$ of voting after being contacted; and if each initial contact urges an average of n others to vote, boosting their turnout rate by τ, then the vote yield Y(k) will be at least $k(\delta + n\tau)$.[14] Because n reflects the average social connectedness of those con-

[13] We use "social density" as a synonym for social capital at the aggregate level. Social capital has two components (at the individual level): social connectedness (being plugged into social networks, having many contacts with whom repeated interactions occur) and interpersonal trust (reflecting low transaction costs within the networks or a "cooperative disposition" fostering a sense of mutual obligation). See Rosenstone and Hansen (fn. 3); John Brehm and Wendy M. Rahn, "Individual-Level Evidence for the Causes and Consequences of Social Capital," *American Journal of Political Science* (forthcoming). The text below provides one way to formalize these notions.

[14] Suppose there are n voters and let $c_j = 1$ if voter j is contacted by the focal candidate, 0 otherwise. Let δ_j be the increment to j's probability of turning out, if he is contacted; p_{jk} be the increment to the probability that j will urge k to vote, due to j's having been mobilized by the candidate; τ_{kj} be the

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tacted (along with their propensity to urge others to vote), while τ reflects the interpersonal trust within the networks into which those contacted are plugged (along with their persuasiveness), the average vote yield per contact, $y = Y(k)/k$, will be bounded below by a direct function of the social capital (connectedness and trust) of those contacted.

Now imagine a pair of candidates—Sato and Tanaka—competing for the last available seat in a district. How much should these candidates mobilize as a function of the social capital and closeness of their district? To save notational clutter and focus on interdistrict rather than intradistrict differences, we suppose that both Sato and Tanaka have an equal number K of mobilizable supporters, each with an expected vote yield of y; that it costs c to contact any given supporter; and that each candidate's utility for winning a seat is B.[15] Under the hypothetical scenario that neither candidate mobilizes any of his/her supporters, the number of votes that they will receive in the forthcoming election is of course not known with certainty. We presume that the candidates have shared beliefs about the probability distribution that would govern their vote totals in this zero-mobilization scenario. In particular, letting V_S and V_T denote random variables representing the total votes that Sato and Tanaka would receive, we represent the two candidates' beliefs about their likely vote totals with a joint probability distribution function g (where $g(x,y)$ gives the probability that $V_S = x$ and $V_T = y$).

Each candidate chooses a number of supporters to contact—say, k_S for Sato and k_T for Tanaka. Contacting more supporters is good in that it increases the candidate's probability of winning a seat, but bad in that it is costly. We assume that each candidate balances these considerations, choosing k in order to maximize (the increase in probability of victory that mobilizing k more supporters produces) × (the value of winning a seat), less (the costs of contacting k more supporters). If one focuses on the simplest case, in which either Tanaka will win the last-

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increment to k's probability of turning out, if he is contacted by j. For a given decision about whom to contact—$(c_1,...,c_n)$—the vote yield $Y(c_1,...,c_n)$ is $\sum_j c_j \left(\delta_j + \sum_{k \neq j} p_{jk} \tau_{kj} \right)$. If p_{jk} and τ_{kj} correlate positively (j is more likely to contact those who are more likely to be persuaded to turn out by his urging), c_j correlates positively with δ_j (the focal candidate is more likely to contact those more likely to be persuaded to turn out by his appeals), and c_j correlates positively with $p_{jk}\tau_{kj}$ (the focal candidate is more likely to contact those who produce more secondary bang for the buck), then the lower bound stated in the text holds.

[15] We assume that higher values of y are dominated by the secondary mobilization terms rather than by the primary mobilization term. Rosenstone and Hansen (fn. 3) and Robert Huckfeldt and John Sprague argue that secondary mobilization effects dominate even in the U.S., a largely untraditional "mass" society. See Huckfeldt and Sprague, "Political Parties and Electoral Mobilization: Political Structure, Social Structure, and the Party Canvass," *American Political Science Review* 86 (March 1992).

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allocated seat and Sato will be the first losing candidate, or vice versa (so that no other candidates need be brought into the picture explicitly), then Tanaka's objective (for any given k_S) can be written formally as "Choose the k_T that maximizes $[\Pr(V_T + k_T y > V_S + k_S y) - \Pr(V_T > V_S + k_S y)]B - ck_T$."[16]

Letting F denote the cumulative distribution function governing $V_S - V_T$, the gap between Sato and Tanaka, Tanaka's utility function can be rewritten as $U_T = F(k_T y - k_S y) - ck_T/B$.[17] Consider now the situation before either candidate has started to mobilize ($k_T = k_S = 0$). In this case, Tanaka will wish to contact some positive number of supporters if $f(0)y > c/B$ (where f is the probability density associated with F).[18] The left-hand side of this last expression is the product of a measure of closeness (namely, f(0)) and a measure of social capital (namely, y). That the vote yield per supporter contacted (y) can be taken as reflecting social capital has been indicated above. That f(0) is a measure of the closeness of the race for the last seat can be made clearer as follows: if the expected value of $V_S - V_T$ is large in absolute value, then a vote gap of zero will be out in the tails of the distribution, which is to say that f(0) will be relatively small; if, however, the expected value of $V_S - V_T$ is small in absolute value, then a vote gap of zero will be near the center of the distribution, which is to say that f(0) will be relatively large. All told, then, f(0)y is a proxy for the relevant theoretical concept, the product of closeness and social capital.

If this proxy is large, then Tanaka and Sato will both wish to contact some supporters. How many will they contact? If the candidates have very precise notions about the distribution of the gap between their vote shares and are able to say with substantial confidence that, say, 720 more votes will leave them with a very poor chance of winning while 750 more votes will push their probability of victory essentially to unity, then they will mobilize just enough to win the seat with high probability. In the example just given, for example, there would be no point in contacting more supporters in search of votes 751, 752 and 753, because these votes would not improve the candidate's chances of winning (already near 100 percent) but would cost something to produce. In

[16] Note that we focus on the simplest case in which the vote yield of k contacts is known with certainty: it is just ky. Allowing for risky returns to contacting does not change anything essential.

[17] Because k_S is taken as given in Tanaka's problem, one can renormalize her utility function by adding the constant $\Pr(V_T > V_S + k_S y)B$, and dividing by the constant B, yielding $\Pr(V_T + k_T y > V_S + k_S y) - ck_T/B$. A little algebra then yields the formulation in the text.

[18] Tanaka will wish to contact some positive number of supporters if the marginal utility of contacting another supporter is positive, when $k_T = 0$. Since $dU_T/dk_T = f(k_T y - k_S y)y - c/B$, Tanaka's marginal utility is positive when $f(-k_S y)y > c/B$, which reduces to the expression in the text when $k_S = 0$.

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cases where the candidates have very precise beliefs about the vote gap separating them, the closer the race is, the *fewer* contacts the candidates make. We believe that, even in these cases, the total number of voters mobilized (ky) will increase with closeness, if the possibility of a sequence of mobilizations and countermobilizations is allowed.

But this sort of result is easier to see in what is arguably a much more prevalent case: the one in which candidates are uncertain enough about the distribution of $V_S - V_T$ (that is, the distribution has a large enough variance) and the number of mobilizable supporters K is small enough so that f(0) is not greatly different from f(Ky). In words, the candidates' uncertainty about the size of the likely gap that will separate them is large relative to the number of voters they are currently thinking of contacting. In this case, each candidate's optimal choice is either to contact no one at all (if the race is not sufficiently close relative to the maximum vote yield K) or to contact all K (otherwise). Thus, the total number contacted increases with the product f(0)y. For $f(0)y < c/B$, neither candidate mobilizes. For $f(0)y > c/B$, both candidates mobilize all K of their supporters.

The purpose of the simple model just outlined is to sustain the proposition that elite mobilizational effort varies directly with the product of social capital and closeness. We turn now to consider how one might test this interaction hypothesis.

Unfortunately, we do not have a measure of mobilizational effort that is appropriate for testing the interaction hypothesis *directly*. Our measure of effort in a district, discussed further below, is the total of campaign expenditures in that district. While expenditures fuel some kinds of mobilizational efforts, however, they may well be unrelated to others.

In societies with strong social networks, secondary mobilization effects—analogous in some way to word-of-mouth advertising—may be most effectively activated by the candidate himself or herself. In the extreme, if a few people have very large nτ's— they are vote brokers capable of mobilizing "their" people—then mobilization does not require mass appeals funded by campaign expenditure, such as door-to-door canvassing or phone bank solicitations. It requires a few direct contacts and a lot of effort per contact (with much of that effort probably expended prior to the start of the campaign). Even if there are not vote brokers properly speaking, if the primary mobilizational strategy is personally calling for help from a network of supporters, then again the level of campaign expenditures may only weakly correlate with mobilizational effort. At the other extreme, if society is unorganized, with

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$n\tau$ near zero for everyone, then there is no point in personal contacts. It is time for college volunteers knocking on doors or paid workers calling targeted area codes and prefixes; these sorts of activities are more plausibly fueled by campaign expenditures.

All told, then, one expects mobilizational strategy to be more personalized (and less campaign-expenditure intensive) in districts with higher social capital and to be more anonymous (and more campaign-expenditure intensive) in districts with lower social capital. Spending plausibly will go up in closer races, since mass mobilization will form part of a good overall mobilizational strategy in most districts. But there is no clear reason to expect that it will go up in districts with higher social capital. The relationship between *spending* and the product of closeness and social capital could thus be very different from the relationship between *mobilizational effort* and this product. So how can we test the interaction hypothesis?

Fortunately, we can test it *indirectly*, because turnout is a direct function of the level of elite mobilization.[19] If the level of mobilizational effort is truly an interactive function of closeness and social density, then turnout should also depend interactively on closeness and social density. Because turnout is easily measured, we can test the interaction hypothesis with turnout as the dependent variable, where we cannot with mobilizational effort (lacking a sufficiently good proxy).

Consider some of the consequences of our hypothesis. If turnout really is sensitive to the product of closeness and social density, then when the election is not close and elites have little reason to mobilize, the correlation between turnout and (proxies for) social capital should attenuate. Similarly, in districts that are less socially dense, where elites have poorer targets for mobilization, the correlation between closeness and turnout should attenuate. Thus, if turnout varies according to the multiplicative product of social density and closeness, *turnout will be most sensitive to closeness in socially dense districts, and most sensitive to social density in districts with close races.*[20]

The strategic elites model is abstract enough to apply in various contexts. Evidence showing that closeness boosts elite effort, which in turn boosts turnout, is already available in studies of the U.S., Britain, and Canada.[21] To our knowledge, however, statistical analyses of the inter-

[19] Rosenstone and Hansen (fn. 3).

[20] Note that the pivotal voter model, which posits direct effects of closeness on voter behavior (unmediated by elite mobilizing efforts), does not predict these results.

[21] David Denver and Gordon Hands, "Marginality and Turnout in British General Elections," *British Journal of Political Science* 4 (January 1974); idem, "Marginality and Turnout in General Elections in the 1970s," *British Journal of Political Science* 15 (July 1985); Neil Berch, "Another Look at

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play between closeness, social networks, mobilizational effort, and turnout have not been performed even in the Anglo-American democracies, much less elsewhere. In the remaining sections of the paper, we examine this interplay in Japan. We first document the central role of elite mobilization and social networks in the literature on turnout in Japan, then adapt the strategic elites model to the Japanese context and test its key predictions.

Mobilization in Japan

Much of the literature on voting in Japan clearly focuses on the role of groups and mobilization, rather than on atomistic calculations by individual voters. Candidates' personal support groups, or *koenkai*, are said to play the lead role in mobilizing votes, by lobbying such organizations as local agricultural cooperatives, small businessmen's associations, or neighborhood associations to deliver their members' votes at election time.[22] By the time he is an established politician, a typical Diet member from the Liberal Democratic Party (LDP)—the governing party from 1955 to 1993—supports fifty to eighty constituency organizations, ranging from current-events discussion groups and sports clubs to women's activity circles and social groups for the elderly. These groups meet occasionally to eat and drink, go on outings to hot springs, celebrate the New Year with the Diet member, and so on. In election years group leaders ask members to mobilize their friends to vote for the sponsoring Diet member.[23]

The role of groups in mobilization is also evident in anecdotes concerning the impact of election forecasts on the outcome. The overwhelming sense one gets from local lore is that predictions have a larger effect on campaign organizers and workers than on voters. A typical case is Hokkaido 4 (a five-member, seven-candidate district) in 1986. When the newspapers ranked Democratic Socialist incumbent Kodaira

Closeness and Turnout: The Case of the 1979 and 1980 Canadian Elections," *Political Research Quarterly* 46 (June 1993); Matsusaka and Palda (fn. 2).

[22] Joji Watanuki, "Patterns of Politics in Present-day Japan," in Seymour Martin Lipset and Stein Rokkan, eds., *Party Systems and Voter Alignments* (New York: Free Press, 1967); Gerald L. Curtis, *Election Campaigning Japanese Style* (New York: Columbia University Press, 1971); Scott C. Flanagan and Bradley M. Richardson, *Japanese Electoral Behavior: Social Cleavages, Social Networks, and Partisanship* (London: Sage, 1977); Anson Shupe, "Social Participation and Voting Turnout: The Case of Japan," *Comparative Political Studies* 12 (July 1979).

[23] Hiroto Sasago, Kazuyoshi Abe, and Katsuya Muraoka, *Seiji kenkin no kozu* (The structure of political finances) (Tokyo: JICC shuppan kai, 1990), 59–60; Masumi Ishikawa and Michisada Hirose, *Jiminto: choki shihai no kozo* (The LDP: The structure of long-term dominance) (Tokyo: Iwanami shoten, 1989); J. Mark Ramseyer and Frances Rosenbluth, *Japan's Political Marketplace* (Cambridge: Harvard University Press, 1993).

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Tadashi below the winning threshold some weeks before the election, several of his electoral support groups—including agricultural cooperatives and *Domei*-affiliated unions—shifted into high gear to bring out the vote on his behalf. Instead of losing, Kodaira finished strongly, in second place, while the second-ranked candidate, whose backers thought they were coasting to an easy finish, actually lost.[24]

Groups also play the lead role in "demobilizing" voters. If a group decides that its favorite candidate needs to be punished, it can direct its voters to abstain or devote less effort to mobilizing support this time around. Flanagan calls this "mobilized group defection."[25] (An example is said to be the abstention of the farm vote in the 1989 Upper House elections, in retaliation for the LDP's cutting the producer price of rice in 1987.) According to this view, the effect of issues on turnout is not direct but is mediated by local vote brokers, who decide the level of effort they will devote to mobilization in any given campaign.[26]

The literature clearly suggests that elite mobilizational effort substantially affects turnout in Japanese elections. Yet, although some works seem to imply a relationship between closeness and turnout in Japan, there is almost nothing in the literature that systematically addresses this possibility,[27] and quantitative analyses of the turnout-closeness relationship do not exist.[28] The literature also shows that most mobilizational effort in Japan is not via mass appeals, but rather via personal contacts.

[24] Akira Tsujimura, "Shimbun ga moteasobu 'senkyo yosoku,' I" (Newspapers trifle with "election forecasts," part I), *Shokun* 18 (September 1986). One often hears the laments of the candidates slotted in first place. A politician complained to a newspaper reporter after an election, "My vote total went down by ten thousand votes because you said I was a shoo-in." Ikuo Kabashima, *Seiji Sanka* (Political participation) (Tokyo: Tokyo University Press, 1988), 172. As one campaign organizer explained, "The campaign effort falters as soon as news about your front-runner status gets out. . . . People in the lower reaches of our campaign organization do not think they have to work as hard, and people we are counting on to vote feel less obligation to show up." Akira Tsujimura, "Shimbun ga moteasobu 'senkyo yosoku,' II" (Newspapers trifle with "election forecasts," part II), *Shokun* 18 (October 1986), 188.

[25] Scott C. Flanagan, "Mechanisms of Social Network Influence in Japanese Voting Behavior," in Scott C. Flanagan et al., eds., *The Japanese Voter* (New Haven: Yale University Press, 1991), 453–57.

[26] Bradley Richardson, "The Japanese Voter: Comparing the Explanatory Variables in Electoral Decisions," in Flanagan et al. (fn. 25), 383–84.

[27] Kabashima (fn. 24) mentions in passing that competitiveness and turnout move together; the point is also made by Seizaburo Sato and Tetsuhisa Matsuzaki, *Jiminto seiken* (The LDP administration) (Tokyo: Chuo koron sha, 1986), 116–17.

[28] Research into turnout has focused (1) on changes in national-level turnout over time—Kabashima (fn. 24), 171–80; Ichiro Miyake, *Tohyo Kodo* (Voting behavior) (Tokyo: Tokyo University Press, 1989), 227; Kunio Arai, *Senkyo, Joho, Yoron* (Elections, information, public opinion) (Tokyo: NHK Books, 1988), 32–35; (2) on broad demographic effects—Arai, 84–92; Michael Blaker, *Japan at the Polls* (Washington, D.C.: American Enterprise Institute, 1976), 149; Flanagan (fn. 25), 174–78; Flanagan and Richardson (fn. 22), 37; Ronald J. Hrebenar, *The Japanese Party System: From One-Party Rule to Coalition Government* (Boulder, Colo: Westview Press, 1986), 15–19; and (3) on anecdotal evidence that "nothing apparently draws the voters like a good conservative blood-letting." Nathaniel Thayer, *How the Conservatives Rule Japan* (Princeton: Princeton University Press, 1969), 137.

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Social Networks in Japan

"The women count more anyway. Isn't that true in the U.S.?" So commented a New Frontier Party candidate in rural Gifu, asked by one of the authors why he was going door-to-door during the workday, when all the men were away from home. The role of social networks is already evident in our discussion above of mobilization. In this section, we consider two particular kinds of social networks—those that mobilize women (especially with children) and those that mobilize rural folks. These are the two kinds of social networks that are most frequently and prominently mentioned in the literature as effectively involved in political mobilization.

Mobilizing Mothers

Susan Pharr points out that a greater percentage of voting-age Japanese women than their male counterparts have voted in every national election since 1968.[29] Overall, the turnout differential is typically about 1 or 2 percent, but it ranges as high as 10 percent for voters aged twenty to thirty-nine.[30]

This gender gap in turnout may seem odd, given that traditional Japanese still see a woman's place as very much in the home and that opinion polls typically find fewer women asserting "an interest in politics" than men (60 percent versus 81 percent in one 1990 poll).[31] A closer look at the reasons for higher women's turnout, however, makes the gender gap in turnout more intelligible.

First, the fact that many women claim to be apolitical by no means precludes their turning out to vote in high percentages. As Scott Flanagan notes, Japanese social networks "can transform the voting act into a social obligation," supplying "an apolitical voter not only with name recognition and a favorable image for a particular candidate but also with a strong motivation to vote."[32]

Second, various studies suggest that Japanese women, especially mothers of young children, are involved in organizations and social networks through which they can be more effectively mobilized into political action than can men. Anthropologist Ted Bestor observed in the Miyamoto-cho neighborhood of Tokyo that "[m]ore women than men

[29] Susan J. Pharr, *Political Women in Japan: The Search for a Place in Political Life* (Berkeley: University of California Press, 1981), 25–26.

[30] Ibid., 32.

[31] Sumiko Iwao, *The Japanese Woman: Traditional Image and Changing Reality* (Cambridge: Harvard University Press, 1993), 234.

[32] Flanagan (fn. 25), 158.

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spend their days at home in the neighborhood, and in various ways their roles are more likely to include a strong component of community involvement. At the minimum a married woman with children has established local contacts through the school's PTA and through her shopping.[33] Many community white-collar male residents know even their closest neighbors only by sight. In addition, most women are involved in a variety of local groups and networks, based on schools, on hobbies, and on friendships."[34] Leaders of all kinds of local campaigns and citizens' movements, especially those relating to the safety of foods and household products, are drawn from the pool of concerned housewives.[35] Since women usually talk with other women from the same local area, whereas the prototypical salaryman will usually talk with coworkers drawn from a number of different areas, women have a natural advantage in accumulating political information about local politics. Since lower house elections were formerly held in relatively small districts, this advantage presumably extended to those elections.

Mobilizing Rural Folks

Voter turnout in Japan's rural districts has been consistently higher than in urban districts for every level of elections.[36] Given that urban voters typically express a greater interest in politics in public opinion polls, high rural turnout poses a puzzle that parallels the gender-based turnout differential just discussed.[37] The most plausible explanation for this disconnection between "political interest" and "political action"

[33] In 1975, 66.2 percent of women aged twenty to twenty-four were in the workplace, compared with only 43.9 percent of women aged thirty to thirty-four. In 1990 the corresponding figures were 75.1 percent and 51.7 percent, respectively. So while the number of young women in the workforce has increased over time, the number who quit their jobs when they marry or have children has increased as well. These women, along with those who never enter the workforce, are the "young mothers" to whom we refer, who are active in local social networks. See Iwao (fn. 31), 63. See also Anne E. Imamura, *Urban Japanese Housewives: At Home and in the Community* (Honolulu: University of Hawaii Press, 1987). Imamura (pp. 116–20) points out that membership in the PTA and other groups related to children is typically automatic and that most women feel they have to take a turn as an officer. While many women return to work once their children are older, they tend to stay closer to home or work shorter hours. See also Mary C. Brinton, *Women and the Economic Miracle: Gender and Work in Postwar Japan* (Berkeley: University of California Press, 1992). Brinton reports (p. 176) that only 31 percent of working women aged forty to forty-four work full time, while 34 percent work part time or as temps, 16 percent are self-employed, 15 percent are family enterprise workers, and 4 percent do piecework. And see also Alice Lam, "Equal Employment Opportunities for Japanese Women: Changing Company Practice," in Janet Hunter, ed., *Japanese Women Working* (London: Routledge, 1993).

[34] Theodore C. Bestor, *Neighborhood Tokyo* (Stanford, Calif.: Stanford University Press, 1989).

[35] Iwao (fn. 31), 244.

[36] Bradley Richardson, "Urbanization and Political Participation: The Case of Japan," *American Political Science Review* 67 (June 1973), 436; Nozumu Matsubara and Ikuo Kabashima, "Tanaka ha assho Jiminto taihai no kozu" (The logic of an overwhelming Tanaka Faction victory amidst an LDP defeat), *Chuo koron* 99 (February 1984).

[37] Richardson (fn. 36), 437.

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again relates to the density of social networks. Residents in urban and suburban areas tend to be quite mobile, typically living in large apartment complexes. Urban and suburban husbands, moreover, often commute long distances to work, giving them a variety of interests outside their residential areas. Rural voters, by contrast, are typically long-standing homeowners in stable communities. Neighbors are more likely to see each other frequently and know each other well.[38] And rural residents have "far higher levels of organized group membership and participation," which facilitates mobilization at election time.[39]

The Strategic Elites Model in the Japanese Context

The strategic elites model makes various predictions. In this section, we consider how one might interpret and test three of them in the Japanese context.

Prediction 1: Closeness Boosts Elite Mobilization Effort

One test of this hypothesis conducted in previous work on the U.S., the U.K., and Canada proceeds as follows.[40] "Mobilizational activity" is measured in a given district by the total of reported campaign expenditures for that district (TOTEXP); "closeness" is measured by the vote margin separating the first and second candidates, expressed as a percentage of the total electorate (MARGIN); and the level of mobilizational activity is regressed on closeness and a battery of control variables ($\{Z_j\}$):

$$\text{TOTEXP} = \alpha + \beta\text{MARGIN} + \Sigma\gamma_j Z_j + \varepsilon \qquad (1)$$

The coefficient β tests the impact of closeness on mobilization and is expected to be negative—larger margins mean the race is less close, which should lead to less mobilizational activity (here proxied by expenditures).

In order to adapt this test to the Japanese context, we must measure expenditure, consider how well measured expenditures relate to "mobilizational effort," and measure closeness. We shall consider each task in turn.

Consider first the measurement of campaign expenditures. In Japan reported expenditures by lower house candidates are widely believed to

[38] Flanagan (fn. 25), 154; Richardson (fn. 36), 438.
[39] Richardson (fn. 36), 437–38.
[40] Cox and Munger (fn. 6); Denver and Hands (fn. 21, 1974, 1985); Berch (fn. 21); Matsusaka and Palda (fn. 2).

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grossly understate true expenditures. It is important to note, however, that Japanese candidates file two separate expenditure reports, one covering the very short official campaign period[41] and one covering the interelection period. The latter reports are those believed to be most inaccurate, although a leading scholar claims that the official figures nevertheless accurately reflect the "trend of the relationship between politics and money."[42] We use data for the official campaign period only, which are generally reputed to be more accurate.[43] In a sense, the shortness of the campaign is good for our purposes, because reported campaign expenditures then reflect the *marginal* or last-minute expenditures of the candidates, which may respond to perceived closeness more sensitively than interelection expenditures.

Consider next the relationship between reported campaign expenditures and mobilizational effort. There are two points to note in this regard. First, expenditures are a plausible proxy for elite effort on mass mobilization. In Japan prior to 1994 (when a major electoral reform was passed), phone banks were the main legal way to engage in such mobilization during the campaign—television appeals, door-to-door canvassing, and mass meetings being illegal[44]—and the campaign expenditure reports include lines for both telecommunications and personnel, the main resources needed for phone banks. Second, campaign expenditures are a poor proxy for personal mobilization. This is not just because such mobilization requires the kind of personal visits and long-term nursing that do not make it onto the campaign reports. It is also because mass mobilization (hence expenditures) may even go down when the social density of a district goes up, to be replaced by more personal mobilization. We return to this point in discussing our results.

Consider finally the problem of measuring closeness. The natural measure—analogous to that used in previous studies—is the number of votes separating the *last winning candidate* from the *first loser*, expressed as a percentage of the total electorate. But closeness in this sense may have had less impact in Japan than in the U.S., the U.K., or Canada, due to differences in the electoral system. In Japan prior to 1994, districts typically elected three, four, or five representatives, each voter

[41] The campaign period lasted twenty days for the 1958 through the 1980 elections, fifteen days for the 1983 through the 1990 elections, and fourteen days in 1993.

[42] Tomoaki Iwai, *Seiji shikin no kenkyu* (Research on political financing) (Tokyo: Nihon keizai shimbun-sha, 1990), 66–67.

[43] Personal communications with Home Affairs Ministry bureaucrats and Japanese journalists. For a discussion of the plausibility of campaign period spending reports, see also Gary W. Cox and Michael F. Thies. "The Cost of Intraparty Competition: SNTV and Money Politics in Japan," *Comparative Political Studies* 31 (June 1998).

[44] Hrebenar (fn. 28), 48.

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casting a single nontransferable vote.[45] Thus, a close race between the last winner and first loser may have prompted *those two* candidates to exert more effort without affecting *other* candidates similarly. It would depend on how close these others were to the edge between winning and losing, something that this simple measure cannot tell us. In Appendix 1 we experiment with some alternative measures of closeness, but in the text we stick with the conventional measure. We thus have a cruder proxy for closeness in Japan's multimember districts than in the Anglo-American democracies' single-member districts.[46]

Prediction 2: Effort (Proxied by Expenditures) Boosts Turnout

With measures of closeness and expenditures in hand, the next step is to see how these variables relate to turnout. Assuming that elite efforts at mass mobilization correlate with reported expenditures and that such efforts are effective, closeness should boost turnout. Thus, if one regresses turnout on closeness and a set of control variables that does not include expenditures,

$$\text{TURNOUT} = \phi + \lambda\text{MARGIN} + \Sigma\gamma_j Z_j + \varepsilon, \qquad (2)$$

one expects the coefficient on MARGIN to be statistically significant (as it will capture the indirect effect of closeness, via its impact on the omitted variable "mobilizational effort"). But if one augments equation 2 by adding a proxy for mobilizational effort, TOTEXP:

$$\text{TURNOUT} = \alpha + \beta\text{MARGIN} + \Sigma\gamma_j Z_j + \theta\text{TOTEXP} + \varepsilon, \qquad (3)$$

then the coefficient on MARGIN should reflect only the direct response of the mass electorate to a closer race, if any, plus the response of personal mobilization (to the extent that this response does not correlate well with mass mobilization). The coefficient on MARGIN should thus diminish when TOTEXP is included as a regressor.

[45] In various years there were also a couple of two-seat and six-seat districts and one one-seater. We drop them from the anlaysis for the sake of simplicity. In early 1994 Japan abandoned this electoral system (in use from 1925 through 1993, with a brief interruption in the 1940s) in favor of one combining single-member constituencies with large proportional representation districts.

[46] Another well-known issue is whether ex post measures of closeness correlate highly with ex ante measures, the latter being the theoretically relevant ones. If the election has equilibrated—in the sense that (1) everyone shares the same expectations about who will finish where in the poll (and hence how close the race will be), (2) everyone is behaving optimally in light of those expectations; and (3) optimal behavior is consistent with the postulated expectations—then ex post measures of closeness will simply be realizations of a particular draw from an ex ante distribution. We follow a host of others in assuming that the ex post measure is acceptable if not ideal; see, e.g., most of the twenty-five studies cited in Matsusaka and Palda (fn. 2).

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Previous studies on American, British, and Canadian elections—all contexts in which mass mobilizational techniques arguably dominate—have found more or less substantial reductions in the coefficient on MARGIN, but not a zeroing out. This has left open the question of whether TOTEXP is an imperfect proxy for elite mobilizational activity more broadly construed—as we think—or whether there is in fact a direct mass response to closeness (for example, because citizens overestimate their chances of being pivotal).

PREDICTION 3: TURNOUT = f(CLOSENESS × SOCIAL DENSITY)

A third step in probing the strategic elites model entails testing our predictions about the interaction between the closeness and social density of a district. In order to do this, we need to define social density operationally. Following the discussion in the preceding section, we consider districts with more mothers of young children and more rural folks as being socially denser than districts with more men, urban dwellers, and women without young children. It is easy enough to measure the urbanness of each lower house district: we use the Japanese government's official measure. But we do not have a direct measure of the number of mothers of young children in our data set. Thus we use the percentage of each district that is female and the percentage of each district that is under the age of fifteen as proxies. The latter percentage indicates the presence of parents of young children and is probably much the better indicator, since the variation in the number of children per family in Japan is relatively small.

Our expectations about the interaction between closeness and social density can be tested by augmenting equation 2 with the appropriate interaction terms. For example, we can test the hypothesis that young mothers are more easily mobilized through social networks by interacting district closeness with the population younger than fifteen.

OPERATIONAL TESTS AND RESULTS

In estimating equations 1, 2, and 3 we use data from postwar Japanese elections to compute turnout rates and margins of victory by district and data from the Home Ministry on campaign expenditure.[47] Because expenditure data are available only for the period 1967–90, the study is

[47] For our data, see http://dodgson.ucsd.edu/lij. For the Home Ministry data, see *Jichisho senkyo kyoku. Shuugiin Giin Sosenkyo Saiko Saibansho Saibankan Kokumin shinsa: Kekkacho* (The results of the elections to the House of Representatives and of the national referendum on Supreme Court justices) (Tokyo: Home Affairs Ministry Public Elections Bureau, various years).

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restricted to these years. We have run our analyses both including and excluding data from years in which redistricting occurred (1967, 1976, 1986). Whether these years are included makes no real difference to our conclusions, so we report results only for the full data set.[48] Appendix 2 provides summary statistics for all variables.

The estimation procedure we have used differs slightly from that used in previous studies. Because we have expenditure and vote data for a number of years, we have panel data, a pooled cross section–time series data set. It turns out that we face first-order autocorrelation in each of our analyses, and so in each we have included lags of the dependent variable (which solves the problem).[49]

Results 1: Does Closeness Boost Elite Mobilizational Effort?

Our proxy for elite efforts at mass mobilization is the total amount expended (in constant 1980 yen) by all candidates, divided by the number of registered electors (TOTEXPE). We measure closeness by the margin separating the last winner from the first loser, divided by the total number of registered electors (MARGIN).[50] We divide by the size of the electorate, rather than the number of votes cast, in order to avoid inducing an artifactual relationship between the dependent variable and the measure of closeness.[51]

[48] One reason for worrying about redistricting in the present analysis is that we include the lagged dependent variable in our estimations, and, obviously, this procedure would not make much sense if district number 31 in 1976 were substantially different from district number 31 in 1972. The reason that it does not make much practical difference whether we include redistricting years or not is that the Japanese redistrictings affected only a small minority of districts in any substantial way.

[49] We prefer using the lagged dependent variable to using the Prais-Winsten (or other) GLS estimator for the reasons articulated in Nathaniel Beck and Jonathan N. Katz, "What to Do (and Not to Do) with Time-Series Cross-Section Data," *American Political Science Review* 89 (September 1995), 634–47. Reestimating all our models using Prais-Winsten does not affect any of our conclusions. We report Huber cluster robust standard errors in Tables 1, 2, and 3 (implemented through Stata's cluster and robust options in its "regress" procedure). These are similar to a restricted version of Beck and Katz's panel-corrected standard errors. Use of the latter is not indicated in our case because we have a relatively short time series (T = 8).

[50] Arguably, a 1 percent gap by this measure means something different in districts of varying magnitude (M): 1 percent of the registered electorate is 2 percent of a Droop quota in a one-seat district, but 5 percent of a Droop quota in a four-seat district, for example. We correct for this by dividing by the Droop quota in each district or, equivalently, by multiplying by M + 1. This allows us to pool the observations from districts of varying magnitude and estimate a common slope.

[51] Compare Gary W. Cox "Closeness and Turnout: A Methodological Note," *Journal of Politics* 50 (August 1988). We tried using the margin and margin squared, as suggested by Cox, but found no significant nonlinear effects. Note that Cox's recommendation that a squared term be included matters from a practical standpoint only if the data actually have observations for which the raw vote margin as a percentage of the electorate (M_e) is large. But the largest observed value of M_e is 18 percent in Japan (much lower than in the U.S.). So, theoretically speaking, the relationship between turnout and M_e *could* be negative over the entire range of observations. And, as it turns out, it is: the squared term is never significant and not even consistently positive in our analyses.

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In addition to the key independent variable, MARGIN, we also include several control variables that reflect the political situation in the district (each expected to boost total expenditures): NCPER (the total number of candidates running in the district, per elector), NLDPPER (the number of LDP candidates, per elector), DISTMAG4 (a dummy variable equal to 1 for four-seat districts, equal to 0 otherwise), DISTMAG5 (a dummy variable equal to 1 for five-seat districts, equal to 0 otherwise), and the lagged value of TOTEXPE.[52] We include both the number of candidates and the number of LDP candidates to accommodate the fact that LDP candidates spend more than the candidates of other parties. (Unlike some of its competitors, the LDP lacks an army of volunteers, so its candidates must pay their campaign workers.)[53] Given the way we have defined the variables, the impact on total expenditure of an additional non-LDP candidate is reflected in the coefficient of NCPER, while the impact of an extra LDP candidate is measured by the sum of the coefficients on NCPER and NLDPPER. Finally, we include dummy variables for each election year, to control for idiosyncratic effects of particular elections. Among other things, the year dummies allow us to control for any peculiarities related to the "double elections" in 1980 and 1986, when elections for both the Upper and the Lower House of the Diet were held on the same day.

Our results appear in Table 1. As would be expected, more candidates—and especially more LDP candidates—boost total expenditures. Most important there is a statistically significant tendency for closer elections (elections where MARGIN is smaller) to spark greater expenditure, controlling for the impacts of other regressors. Substantively, the impact of closeness seems to be small: if we posit a decrease of 1 percentage point in the gap separating the last winner from the first loser, then the estimated increase in total expenditure per elector would be 0.96 yen,[54] about a fourth of a cent in 1980.

One reason this effect is so small is that not all candidates need be concerned with the closeness of the race for the last seat, which is what we measure. Candidates who are well ahead of the last winner (or hopelessly behind the first loser) have little incentive to boost spending, because it is the last winner and the first loser (and not they) who are in

[52] In order to bring the magnitude of the coefficients on NCPER and NLDPPER into line with the others in the model, we use the number of candidates per hundred thousand electors, rather than just per elector. This of course affects only the placement of the decimal point in the coefficient estimate.

[53] Hrebenar (fn. 28), 48.

[54] This is calculated by dividing the coefficient on MARGIN by the Droop quota (equivalent to multiplying by M + 1 = 5 for a four-seat district), which gives us .191 × 5 = 0.956. Recall that we standardize the closeness measure so that the same measure can be used in districts of three, four, and five seats.

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TABLE 1
TOTAL EXPENDITURES AS A FUNCTION OF CLOSENESS: JAPAN, 1969–90[a]
(DEPENDENT VARIABLE: TOTEXPE—TOTAL EXPENDITURES IN DISTRICT, PER ELECTOR)

Independent Variable	*Estimate*	*Standard Error*	*T-statistic*	*Prob > |T|*
Intercept	−18.272	2.032	−8.991	0.0001
MARGIN	−0.191	0.034	−5.587	0.0001
NCPER	37.662	1.564	24.085	0.0001
NLDPPER	4.377	2.388	1.628	0.1038
DISTMAG4	2.990	0.954	3.133	0.0018
DISTMAG5	3.400	0.976	3.484	0.0005
Lag(TOTEXPE)	0.305	0.023	13.339	0.0001
1972 Dummy	−0.651	1.583	−0.411	0.6809
1976 Dummy	31.761	1.590	19.978	0.0001
1979 Dummy	36.174	1.816	19.919	0.0001
1980 Dummy	28.363	2.017	14.063	0.0001
1983 Dummy	35.393	1.912	18.509	0.0001
1986 Dummy	37.673	2.009	18.748	0.0001
1990 Dummy	39.618	1.996	19.853	0.0001
Observations	999.0000			
Mean of D.V.	78.741			
Adj. R-square	0.825			

[a] Data for 1967 are not included, because we do not have information on the lagged value of TOTEXPE.

a close battle for the last seat.[55] Another reason may be that candidates generally prefer personal mobilizational techniques and respond to closeness primarily by intensifying their use of these techniques. Finally, there may simply be less variation in closeness in Japan, hence less room for closeness to affect expenditures—a notion to which we return in the conclusion.

RESULTS 2: DOES ELITE MOBILIZATIONAL EFFORT BOOST TURNOUT?

In exploring how much elite mobilizational effort boosts turnout, we run two regressions, corresponding to equations 2 and 3 above. (See Table 2.) Model 1 includes the lagged turnout rate; the percentage

[55] To investigate whether or not the increase in expenditures due to closeness is driven by the candidates actually on the margin, we reestimated equation 1 using the per elector expenditures by the two candidates on the cusp between winning and losing (the last winner and first loser) as the dependent variable. (This specification required some modification of the right-hand-side variables as well.) Our results (available on request) show that the two marginal candidates do indeed account for a disproportionate share of the overall effect.

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TABLE 2
TURNOUT AS A FUNCTION OF CLOSENESS AND EXPENDITURES: JAPAN, 1969–90
(DEPENDENT VARIABLE: TURNOUT)

Independent Variable	*Model 1*			*Model 2*		
	Estimate	*t-Statistic*	*P-Value*	*Estimate*	*t-Statistic*	*P-Value*
Intercept	45.471	2.814	0.006	48.483	2.872	0.005
MARGIN	−0.103	−8.091	0.000	−0.093	−7.644	0.000
TOTEXPE				0.038	3.078	0.003
NCPER	2.139	3.589	0.000	0.602	0.759	0.450
NLDPPER	3.116	2.628	0.010	2.582	2.265	0.025
%Urban	−0.059	−2.903	0.004	−0.060	−3.011	0.003
%pop < 15	0.379	3.340	0.001	0.416	3.680	0.000
%Men	−0.446	−1.703	0.091	−0.495	−1.816	0.072
%pop > 65	0.313	1.251	0.213	0.290	1.166	0.246
Lag (Turnout)	0.456	4.916	0.000	0.445	4.828	0.000
1972 Dummy	5.234	7.472	0.000	5.111	7.316	0.000
1976 Dummy	5.472	9.126	0.000	4.162	5.848	0.000
1979 Dummy	0.530	0.728	0.728	−1.221	−1.451	0.149
1980 Dummy	8.800	8.876	0.000	7.150	6.427	0.000
1983 Dummy	−0.168	−0.199	0.842	−1.860	−2.023	0.045
1986 Dummy	5.400	5.220	0.000	3.506	3.179	0.002
1990 Dummy	6.415	6.085	0.000	4.469	3.814	0.000
Observations	999.000			999.000		
Adj. R-square	0.796			0.825		

urban, percentage of population under fifteen, and percentage of population male (to measure the social density of the district);[56] the percentage of population over sixty-five years of age and the number and type of candidates competing in the election (as controls);[57] and MARGIN. Model 2 includes all of these regressors plus TOTEXPE.

There are three main points to make about our results in this pair of regressions. First, the coefficients on the demographic variables all conform to expectations. In particular, we found that districts containing more young parents, more rural people, and more women had higher levels of turnout.

Second, campaign expenditures do have a statistically and substantively significant impact on turnout rates. Each increase in expenditure of 1 yen per elector increases turnout by .038 percentage points. If dis-

[56] The government's official measure of urbanness is the percentage of the population living in "densely inhabited districts," analogous to the "standard metropolitan statistical area" used in the United States.

[57] We have also used variables tapping the occupational structure of districts as controls, but these turn out to be highly correlated with the urbanness of the district and so are excluded here.

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trict expenditures increased from one standard deviation below the mean to one standard deviation above the mean (a ¥50 to ¥60 per elector difference in the typical year after 1972), turnout would increase on average by 1.9 percent to 2.3 percent. The analogous effect in the United States is about 3.8 percent.[58]

Third, the coefficient on MARGIN does decline when TOTEXPE is included in the regression, from 0.103 to 0.093. Perhaps there is an unmediated mass response to closeness, or perhaps total expenditure is—as argued above—a poor proxy for the full range of mobilizational effort.

RESULTS 3: IS TURNOUT = f(CLOSENESS × SOCIAL DENSITY)?

To explore the interaction between closeness and social density, we augment Model 2 from Table 2 with interaction terms between closeness (MARGIN) and our primary measures of social density (percentage urban and percentage of population younger than fifteen). Table 3 displays three of the specifications we have examined.

Model 1 in the table shows that the interaction between closeness and urbanness is statistically and substantively significant. The results can be interpreted from either of two perspectives. First, to find how much a unit change in closeness affects turnout, one must add the coefficient on MARGIN to the coefficient on the interaction MARGIN × %URBAN, with some particular value of %URBAN stipulated. From this perspective, one sees that closeness has its biggest impact (−.152) when there are no urban areas in the district and its smallest impact—almost nil (−.152 + (100 × 0.00141) = −.011)—when the district is 100 percent urban. Thus, while closeness matters when comparing rural districts with rural districts—a coefficient of −.152 implies that moving from one standard deviation below the mean closeness to one standard deviation above would produce an increase in turnout of 3.6 percent—it matters very little when comparing urban districts with urban districts. Second, to find how much a unit change in urbanness affects turnout one must add the coefficient on %URBAN to the coefficient on the interaction MARGIN × %URBAN, with some particular value of MARGIN stipulated. From the perspective of how closeness conditions the relationship between urbanness and turnout, one sees that urbanness has its biggest impact (−.068) in the closest races (where MARGIN is 0), with

[58] Cox and Munger (fn. 6). The variability in total expenditures is larger in the U.S., because the variability in competition is larger. If one compares the impact of equal expenditures, a different comparison arises. Using an exchange rate of about 250 yen per dollar for the early 1980s, one more dollar (250 more yen) per voter would lead to an increase in turnout of .038 × 250 = 9.5 percent. This compares to a figure of 4.0 percent from Cox and Munger for the U.S. case.

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TABLE 3
TURNOUT AS A FUNCTION OF CLOSENESS AND SOCIAL NETWORKS: JAPAN, 1969–90
(DEPENDENT VARIABLE: TURNOUT)

Independent Variable	*Model 1*		*Model 2*		*Model 3*	
	Estimate	*T-stat*	*Estimate*	*T-stat*	*Estimate*	*T-stat*
Intercept	44.642	2.656	47.113	2.786	43.891	2.615
MARGIN	−0.152	−6.056	0.271	2.023	0.184	1.493
TOTEXPE	0.038	3.069	0.041	3.252	0.040	3.217
NCPER	0.563	0.709	0.486	0.621	0.463	0.589
NLDPPER	2.600	2.274	2.877	2.520	2.863	2.500
%URBAN	−0.068	−3.383	−0.060	−3.024	−0.067	−3.331
MARGIN × %URBAN	0.001	2.773	0.001	2.425		
%pop < 15	0.436	3.883	0.539	4.284	0.545	4.546
MARGIN × %pop < 15	−0.016	−2.764	−0.015	−2.720		
%MEN	−0.430	−1.576	−0.516	−1.873	−0.456	−1.658
%pop > 65	0.358	1.480	0.258	1.039	0.321	1.327
Lag (Turnout)	0.446	4.843	0.442	4.796	0.444	4.812
1972 Dummy	5.029	7.241	5.064	7.327	4.996	7.245
1976 Dummy	4.009	5.699	4.101	5.910	3.973	5.757
1979 Dummy	−1.402	−1.669	−1.365	−1.665	−1.508	−1.842
1980 Dummy	6.939	6.282	7.012	6.383	6.842	6.263
1983 Dummy	−2.077	−2.274	−1.968	−2.178	−2.147	−2.384
1986 Dummy	3.265	2.981	3.330	3.078	3.137	2.909
1990 Dummy	4.138	3.549	4.377	3.786	4.098	3.542
Observations	999		999		999	
Adj. R-square	0.800		0.801		0.802	

the impact steadily declining as MARGIN increases and actually becoming positive for the 3 percent of the districts in which MARGIN exceeds 47 percent. Thus, while increasing the urbanness of a district from one standard deviation below the mean to one standard deviation above depresses turnout by 3.6 percent when one compares very close districts to very close districts, urbanness has essentially no impact on turnout when one compares uncompetitive districts to uncompetitive districts.

Model 2 shows that the interaction between closeness and the percentage of the population younger than fifteen is also statistically and substantively significant. The results can again be interpreted from either of two perspectives. From one perspective, one sees that closeness has its biggest impact when the percentage younger than fifteen is at its maximum (30.9 percent) and its smallest impact when this percentage

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is at its minimum (14.5 percent). In the first case, where social density is higher, the impact of closeness on turnout is −.23 (=.271 − 30.9 × 0.016), while in the latter case it is essentially nil (.271 − 14.5 × 0.016 = +.04). From another perspective, one sees that the presence of children under the age of fifteen (which we take as a proxy for the presence of mothers with young children) matters most in the closest races (.539) and least in the least close races (actually becoming negative for the 5 percent of cases with MARGIN > 34%).

Model 3 shows that the two interactive effects demonstrated in models 1 and 2 appear to be largely independent phenomena: even when both interactions (closeness with percentage urban, closeness with percentage younger than fifteen) are present, both continue to have qualitatively the same effect. This is in contrast to the interaction (not reported) between closeness and the percentage of the district that is male. Although this interaction effect is statistically significant when entered alone, its substantive importance is small; and when entered with either of the other interaction terms (or both), it is not significant.[59]

Conclusion

A straightforward rational calculus suggests that elites will mobilize more when the probability of their effort deciding the outcome of the election is greater. It is this basic insight that motivated V. O. Key to suggest that the often-observed correlation between closeness and turnout is driven by elite actions. But closeness is an incomplete indicator of the relevant probability. The probability of elite action being decisive depends *jointly* on how many votes they need to determine the outcome (how close the election is) and on how many votes their efforts are likely to yield. In this paper, we argue that elite efforts are likely to yield more votes in districts with higher levels of social capital. If true, this means that both elite mobilizational effort and turnout should vary with the product of closeness and social capital, implying that effort and turnout should be most sensitive to closeness in socially dense districts, and most sensitive to social density in close districts. Because we lack an appropriate way to measure total elite effort, we are

[59] Given the substantial changes over time in the percentage of women working, one might wonder whether the impact of the social indicators that we use might change over time. We have investigated this matter and found no systematic change in effect over time. For example, the effect of %pop < 15 does not vary significantly from year to year. This may be due in part to the fact, noted above, that employment change has been much greater among women *without* young children than among women *with* young children.

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unable to test this hypothesis regarding elite effort directly. But we have tested the interaction hypothesis indirectly, using district-level turnout data from Japanese lower house elections.

What we find strongly supports the interactive hypothesis. In particular, we find that a district's social capital has almost no relationship to turnout if one confines the analysis to extremely uncompetitive races, but that it has statistically and substantively significant effects if one compares competitive races to one another. Similarly, how close a race is has almost no effect on turnout if one confines the analysis to districts with low levels of social capital, but it has statistically and substantively significant effects if one compares districts with high levels of social capital. It has long been known that social networks play a key role in mobilizing the vote in Japanese elections. What has not been appreciated is that the extent to which social networks are engaged in mobilization is not a simple reflex of local social structure. Instead, it follows a straightforward instrumental calculus, the ebb and flow of which can be traced quite clearly in the aggregate election returns.

From a broader comparative perspective, our results bear on the notions that (1) the proportion of districts that are competitive rises with district magnitude (that is, the number of seats per district) and (2) greater competitiveness boosts elite mobilization, hence turnout. Using total expenditures as a proxy for elite effort we find that the closeness of the competition in a particular district does affect expenditures and that greater expenditures in turn boost turnout—which bears on point 2. We can also state that closeness declines significantly, both in mean and in variance, with district magnitude. This pattern is guaranteed, given any reasonable assumptions about vote distributions, but it may be of some interest to report what the figures look like quantitatively: The mean margin separating the last winner from the first loser, expressed as a percentage of the Droop quota (the number of votes it takes to guarantee a seat), declines from 12.4 percent to 9.3 percent to 6.5 percent as district magnitude increases from 3 to 4 to 5. The standard deviation of the margin declines from 14.8 to 10.6 to 7.4 as district magnitude increases from 3 to 4 to 5. Thus, districts in Japan do become significantly more competitive—and more uniformly competitive—as district magnitude increases.

There are several directions for future work. First, do interaction effects analogous to those that we have found for Japan appear elsewhere? Presumably such investigations must be conducted in systems with relatively low district magnitudes, such as the U.S., the U.K., Canada,

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Spain, or Chile. Second, can we develop better measures of informal mobilizational efforts so that more direct hypothesis tests than those shown here can be performed? Third, at what district magnitude does the level and variability of closeness become so low that cross-sectional closeness effects are no longer visible? Fourth, what other predictions can be generated theoretically from the strategic elites model? The pivotal voter model has attracted enormous attention, both positive and negative, and has typically been described as if it were the sole rational choice approach to the study of turnout, upon which the whole rational choice enterprise might stand or fall.[60] Although the pivotal voter model remains an important benchmark, more effort—both critical and developmental—should be devoted to the strategic elites model.

Appendix 1: Alternative Measures of Closeness

The margin separating the last winner from the first loser may be a poor measure of how motivated candidates other than these two are to get out the vote. In principle, what matters for each candidate is how far he or she is from the edge between winning and losing. In order to investigate the impact of closeness on other candidates, we have calculated a margin of victory for each. In a three-seat district with five candidates finishing with vote totals $t_1 > t_2 > t_3 > t_4 > t_5$, for example, the margins of victory for the three winners are $M_{1stwinner-1stloser} = t_1 - t_4$, $M_{2dwinner-1stloser} = t_2 - t_4$, and $M_{lastwinner-1stloser} = t_3 - t_4$, while the margins of victory for the two losers (both negative) are $M_{1stloser-lastwinner} = t_4 - t_3$ and $M_{2dloser-lastwinner} = t_5 - t_3$. Note that $M_{lastwinner-1stloser}$ is the traditional measure of closeness—what we have denoted MARGIN. The candidates who are next closest to the edge between winning and losing are the second winner (in a three-seat district) and the second loser. Thus, in other regressions we have included $M_{2dwinner-1stloser}$ and $M_{2dloser-lastwinner}$ in the analyses, in addition to MARGIN. Because $M_{2dloser-lastwinner}$ is undefined in case there are exactly N + 1 candidates in an N-seat district, we restrict these regressions to cases where the number of candidates exceeds N + 1 (this eliminates 101 out of 999 observations). Before looking at the results, note that the three closeness measures must be closely related. In particular, if either $M_{2dloser-lastwinner}$ or $M_{2dwinner-1stloser}$ is close to zero, then necessarily so must MARGIN be close to zero. Thus, to the extent that $M_{2dloser-lastwinner}$ or $M_{2dwinner-1stloser}$ turn out to have significant effects, one would expect MARGIN's effect to be diminished.

[60] Riker and Ordeshook (fn. 8); Donald Green and Ian Shapiro, *Pathologies of Rational Choice Theory* (New Haven: Yale University Press, 1994).

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TABLE 4
ALTERNATIVE MEASURES OF CLOSENESS AND TOTAL EXPENDITURES
(DEPENDENT VARIABLE: TOTEXPE—TOTAL EXPENDITURES IN DISTRICT, PER ELECTOR)

Independent Variable [a]	*Estimate*	*Standard Error*	*T-Ratio*	*P-Value*
Intercept	−14.346	2.287	−6.273	0.0001
MARGIN	0.096	0.100	0.962	0.3363
$M_{nextwinner-toploser}$	−0.090	0.084	−1.073	0.2834
$M_{lastwinner-2dloser}$	−0.174	0.024	−7.140	0.0001
DISTMAG4	1.848	1.016	1.819	0.0692
DISTMAG5	2.278	1.045	2.181	0.0295
NCPER	34.123	1.388	20.210	0.0001
NLDPPER	11.394	2.955	3.856	0.0001
Lag(TOTEXPE)	0.326	0.024	13.610	0.0001
Observations[b]	898			
Adj. R-square	.832			

[a] To save space, estimates for Election Year Dummy Variables are not reported here.
[b] Number of observations drops from 999 to 898 due to cases with no second loser.

In Table 4 we include all three margins in a regression predicting TOTEXPE. To make the table easier to read, we use the absolute value of all margins (so $M_{2dloser-lastwinner}$ is now called $M_{lastwinner-2dloser}$); the prediction, therefore, is that as each margin gets larger, TOTEXPE should decline. As it turns out, the coefficients on $M_{2dwinner-1stloser}$ and $M_{lastwinner-2dloser}$ are of the correct sign though the former is not significant, while the coefficient on MARGIN is insignificant and of the wrong sign. Because the correlation between $M_{2dwinner-1stloser}$ and MARGIN is .92, it is not too surprising that neither turns out to be significant.

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APPENDIX 2
TABLE 5
SUMMARY STATISTICS

Variable	*Obs*	*Mean*	*StdDev*	*Minimum*	*Maximum*
%URBAN[a]	999	51.5662	26.8267	8.0553	100.0000
%pop > 65[b]	999	9.5999	2.4704	3.9173	16.1388
%pop < 15[b]	999	22.8188	2.3060	14.4730	30.8600
%MEN[b]	999	48.8534	1.1638	45.6861	52.0727
NCPER[c]	999	1.2299	0.4422	0.3707	2.9159
NLDPPER[c]	999	0.4836	0.2498	0.0629	1.4730
MARGIN[c]	999	9.4328	11.6861	0.0112	73.0386
TURNOUT[c]	999	72.5911	8.5196	46.8600	89.7100
TOTEXPE[d]	999	78.7408	28.9139	19.0651	233.4103

[a] %URBAN is the Japanese government's official measure of urbanness, the percentage of the district's population that lives in a "densely inhabited district," akin to the "standard metropolitan statistical area" measure used by the U.S. government.

[b] %pop > 65, %pop < 15, and %MEN are all taken from Japanese census data *(Nippon no Jinko)* as found in the *Rebaisan* Databank's *Shikuchoson-betsu shuugiin deeta* (House of Representatives data by city-ward-town-village), compiled and provided by Ikuo Kabashima of Tsukuba University.

[c] NCPER, NLDPPER, MARGIN and TURNOUT are taken from official electoral returns, available at the Lijphart Elections Archive's web site: http://dodgson.ucsd.edu/lij.

[d] TOTEXPE is taken from official Home Affairs Ministry on campaign expenditures; *Jichisho senkyo kyoku* (various years).

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Review Article

DEMOCRACY AND DICTATORSHIP IN INTERWAR WESTERN EUROPE REVISITED

By THOMAS ERTMAN*

Ruth Berins Collier and James Mahoney. *Labor and Democratization: Comparing the First and Third Waves in Europe and Latin America*, Working Paper no. 62. Institute of Industrial Relations. University of California Berkeley, May 1995, 65 pp.

Gregory Luebbert. *Liberalism, Fascism or Social Democracy*. New York: Oxford University Press, 1991, 416 pp.

Michael Mann. *The Sources of Social Power*. Vol. 2, *The Rise of Classes and Nation-States, 1760–1914*. Cambridge: Cambridge University Press, 1993, 826 pp.

Dietrich Rueschemeyer, Evelyne Huber Stephens, and John Stephens. *Capitalist Development and Democracy*. Cambridge: Polity Press, 1992, 387 pp.

WHY did all the nations of Western Europe become democratic between 1848 and 1921, and why were so many of these young democracies able to survive the crises of the interwar period unscathed? This may seem a surprising question, for far more social scientific and historical energy has been expended on explaining the region's spectacular cases of democratic failure (Germany, Italy, and Spain) than on accounting for its democratic successes—and understandably so given the horrors that Nazi Germany and its allies inflicted upon the Continent. Yet as the experience of recent decades teaches us more about the factors that contribute to the appearance and survival of democratic regimes, the early breakthrough and ultimate durability of democracy in Switzerland, France, Britain, Belgium, the Netherlands, and the three Scandinavian monarchies appears all the more remarkable.

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We possess today a sizable and growing body of literature that seeks to specify the conditions under which democracies will flourish. Summarizing evidence derived from a study of seventy-one countries over a century and a half, Samuel Huntington stresses the significance of a previous experience with democracy, of a favorable international environment, and of a high level of socioeconomic development in promoting the successful consolidation of new democratic regimes.[1] Adam Przeworski and Fernando Limongi's recent statistical analysis[2] of 224 regimes in 135 countries between 1950 and 1990 convincingly confirms the overriding importance of this last factor, famously singled out by Seymour Martin Lipset, who nearly four decades ago asserted that "the more well-to-do a nation, the greater the chance it will sustain democracy."[3] Przeworski and Limongi also highlight the positive role played by rapid economic growth in democratic consolidation and conversely point to the acute threat that sudden economic crises, with their attendant loss of income, pose to young democracies, especially those at lower levels of economic development.[4]

Yet none of these conditions favoring democratic consolidation obtained during the interwar period in Western Europe.[5] Nearly all of that region's states had only recently embarked upon their first experiment with full democracy—and after 1918 the ineffectualness of the League of Nations, the isolationism of the United States, and the postwar weakness of Britain and France meant that little support for such experiments was forthcoming from the international community. The interwar years were a time, moreover, when communism and Fascism/Nazism found wide support as possible alternatives to democracy.

[1] Huntington, *The Third Wave: Democratization in the Late Twentieth Century* (Norman: University of Oklahoma Press, 1991), 14, 270–74.

[2] Przeworski and Limongi, "Modernization: Theories and Facts," *World Politics* 49 (January 1997).

[3] Lipset, *Political Man*, 2d ed. (London: Heinemann, 1983), 31.

[4] Przeworski and Limongi (fn. 2), 167–69, 177. See also Adam Przeworski et al., *Sustainable Democracy* (Cambridge: Cambridge University Press, 1995), 11.

[5] Western Europe lends itself especially well to the study of variation in the process of democratization. During the nineteenth and early twentieth centuries all of the states located there—with the exception of Finland, Ireland, and Austria—were autonomous polities with borders largely unchanged from 1815 through 1939, that is, throughout the whole period of liberalization and first-wave democratization. The states newly created after 1918, whether in Western, Central, or Eastern Europe, faced very different problems of democratization and democratic consolidation compared with the longer-established states of Western Europe and hence, in my view, should be treated as a separate universe of cases (on this issue, see also the discussion in fn. 7). In addition, those twelve longer-established Western European polities—Great Britain, France, the Netherlands, Belgium, Switzerland, Denmark, Sweden, Norway, Germany, Italy, Spain, and Portugal—shared a common cultural, religious, and political heritage that set them apart from the Orthodox and Muslim lands to the east and southeast. Since it is important to hold as many factors constant as possible when attempting to explain divergent outcomes, the commonalities just mentioned speak in favor of analyzing together the fates of the twelve longer-established Western European states during the interwar period.

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Nor was the per capita income of even the wealthiest of Western European states between the wars at a level high enough to guarantee the durability of democratic systems.[6] And, furthermore, all of the Continent suffered sharp losses of income at some point during the 1920s and 1930s due to hyperinflation or depression—or sometimes both.

Hence, given the unfavorable international and ideological climate, their modest level of socioeconomic development, and victimization by a severe economic crisis, Western Europe's first-wave democracies were highly susceptible to breakdown. Yet only four[7] (Portugal, Italy, Germany, Spain) suffered such a fate, whereas the other eight did not. How can this pattern of outcomes be explained?

Three recent and very ambitious books, as well as a substantial working paper, cast new light on this issue. These works are Michael Mann, *The Sources of Social Power,* volume 2, *The Rise of Classes and Nation-States, 1760–1914,* Dietrich Rueschemeyer, Evelyne Huber Stephens, and John Stephens, *Capitalist Development and Democracy,* Gregory Luebbert, *Liberalism, Fascism or Social Democracy,* and Ruth Berins Collier and James Mahoney, *Labor and Democratization: Comparing the First and Third Waves in Europe and Latin America.* Mann's massive tome, the second volume of a projected four-volume work, argues for the importance of geomilitary competition, in addition to the more familiar forces of class conflict, nationalism, and political reform, in shaping the diverse destinies of the great powers over the course of the "long nineteenth century." While rich in its range of insights, the book focuses principally on only three Western European states and emphasizes the historical particularity of each of these cases. As a result, it is not well suited to contribute to a general theory of European

[6] Przeworski and Limongi have found that "[t]he probability that a democracy will die during any particular year in a country with an income above $4,000 [in 1985 U.S. dollars at purchasing power parity] is practically zero," yet the GDP per capita of all Western European countries throughout the interwar period appears to have been below this level, lying instead between about $1,500 and $3,500. See Przeworski and Limongi (fn. 2), 166, 173; and also Paul Bairoch, "Europe's Gross National Product, 1800–1975," *Journal of European Economic History* 5 (Fall 1976), 296–97.

[7] To these four could be added interwar Austria, but the literature is divided on whether to assign it to "Western Europe" (as Rueschemeyer, Stephens, and Stephens and Rokkan do) or "Eastern Europe" (as does Luebbert). It seems to me that excluding the Austrian case from a comparative study of first-wave democratization and democratic durability in Western Europe before 1939 is the more reasonable option. I say this not because Austria is part of the "East" but because, like Ireland, Finland, Czechoslovakia, Poland, and Hungary (all of which arguably belonged to the West), it was essentially a new state created in the aftermath of World War I. It was very different in its borders, population, and form of government from prewar Austria-Hungary. In general, I find the argument convincing that such new post-1918 states faced very different pressures and challenges during the interwar years than did those states already in existence before the war and that therefore they should be examined separately. (See fn. 5.) Hence while the Austrian case will be mentioned in my discussion of Rueschemeyer, Stephens, and Stephens's and Rokkan's work, it will not be included in my concluding theoretical suggestions.

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democratization or of democratic survival and breakdown during the interwar period.

By contrast, Rueschemeyer, Stephens, and Stephens have designed their work to do just this. It can be seen as an attempt to confirm and extend Barrington Moore's insights on the social origins of dictatorship and democracy by first judiciously amending his key contentions and then testing them against some thirty-eight cases drawn from a variety of regions. For our purposes, the most important result is a much more extensive and systematic treatment of European democratization than that found in Moore's classic work. Nevertheless, their book suffers from incongruities between its analysis and its impressive empirical base. Berins Collier and Mahoney point out some of these incongruities and as a consequence reject the claim by Rueschemeyer, Stephens, and Stephens that the working class played the leading role in European democratization. They favor instead an account that stresses the significance of political entrepreneurship and middle-class as well as working-class pressure. Meanwhile, Leubbert indirectly attacks the second part of Rueschemeyer, Stephens, and Stephens's argument by rejecting Moore's claim, which they endorse, that it was above all the presence or absence of a powerful landed elite which determined whether a given European state would become fascist or remain democratic during the 1920s and 1930s. In seeking to arrive at an alternative explanation, he nonetheless follows Rueschemeyer, Stephens, and Stephens (and Stein Rokkan) in examining nearly the entire universe of Western European cases in order to uncover general causal mechanisms. He concludes, however, that it was the ability or inability of prodemocratic political parties to form stable majorities which ultimately determined the fate of democracy in a particular country. Yet while Luebbert is surely right to look to the prewar period to account for interwar outcomes, his attempt to ascribe those outcomes to different patterns of incorporation of the urban and rural working class is ultimately not convincing.

This essay discusses in more detail the strengths and limitations of these four works. In pointing to their limitations, I by no means wish to detract from their real significance, still less to argue that the process of democratization in nineteenth- and early-twentieth-century Western Europe is simply too complex and/or historically contingent to allow for parsimonious explanation.

At the end of this essay, then, I will suggest that before succumbing to such a counsel of despair, we consider an alternative explanatory framework that focuses on divergent patterns in the relationships between

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civil society—conceived of narrowly as associational life—and political parties across the full range of Western European cases. Such a focus may offer some insights into the conditions that help democracy to survive even under relatively unfavorable circumstances such as those prevalent in interwar Europe.

I. Michael Mann

Three features set Mann's book apart from the other works that I will examine. First, it covers only the period up until 1914; a subsequent volume will extend the story through the remainder of this century. Second, though the Scandinavian, Italian, and Spanish cases are mentioned in passing, Mann's comparative framework concentrates principally on Europe's "great powers": Britain, France, Germany, Austria-Hungary, and Russia, as well as on the United States. Finally, the author is as interested in elucidating general processes like the emergence of the modern state and the spread of the industrial revolution as he is in delineating and explaining differences among the principal European polities. Nevertheless, an account of why, for example, the British and French political systems differed fundamentally from that of Germany before 1914 does emerge—an account that is surely relevant to an understanding of the fate of democracy in these nations both before and after 1914.

In volume 1 of *The Sources of Social Power*, Mann stressed the role of geopolitical competition in shaping the emerging states in Europe during the early modern period. In accordance with older authorities like Otto Hintze and contemporaries like Charles Tilly, he argued that the exposed position of France and the German territorial states, surrounded as they were by potential enemies, furthered the emergence there of absolutism during the seventeenth and eighteenth centuries. Conversely, Britain's geographic isolation, protected from the land armies of the Continent by the natural barrier of the English Channel, made possible the preservation of a constitutional form of government, albeit one tainted by corruption and lacking the bureaucratic infrastructure of its continental neighbors.[8]

In this second volume, Mann of course acknowledges the extent to which both the French and industrial revolutions ushered in a qualitatively new period of European and world development. Yet in contrast

[8] These and other views of Mann, Tilly, and Hintze on the development of the European state are examined critically in Thomas Ertman, *Birth of the Leviathan: Building States and Regimes in Medieval and Early Modern Europe* (Cambridge: Cambridge University Press, 1997), chap. 1.

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to those who would see class or nation supplanting geopolitical competition as the new master variables during the long nineteenth century, Mann insists that war and preparations for war continued to shape the way in which these new forces affected the various European powers. Thus he argues (pp. 224–25) that fiscal-military pressures and, later, mass mobilizations for war were the principal mechanism through which classes were politicized and protonations became self-conscious nations during the first part of the century. Furthermore, "democratic ideals were born of war" (p. 225). Only during the second half of the century did industrial capitalism supplant militarism as the driving force behind the further development of states and nations (pp. 730–35).

In general terms, the continued dynamic of geopolitical and industrial competition, combined with the emergence of classes and nations, resulted in what Mann terms "the rise of the modern state." This involved increases in the size, scope, and bureaucratization of the state, along with substantial shifts in the direction of its activities. Thus military outlays decreased as a percentage of total state spending during the course of the nineteenth century and the armed forces became more professionalized and isolated from civilian society. By contrast, the relative weight of civilian spending rose from an average of about 25 percent of all state expenditure in the 1760s to about 75 percent by the early 1900s, as governments began to direct more resources toward infrastructure, education, and social welfare (pp. 370–75, 730). New programs were paid for by revenues provided by higher economic growth, but this growth outpaced even the rate of increase in government spending, leaving the new European state of the early twentieth century actually smaller as a percentage of GNP than its late-eighteenth-century predecessor (pp. 361, 368).

In most cases, this state was able to mold its citizens into a national community thanks to the increasing social interaction made possible by transportation links, universal schooling, and military service. At the same time, the emerging middle and working classes succeeded in gaining expanded political representation within national legislatures, which became newly responsive to their citizenry (pp. 727–35).

Despite these common trends, Mann also highlights the continuing differences in the paths of development pursued by Western Europe's great powers—Britain, France, and Germany. In Britain the industrial revolution served to reinforce the constitutional dualism between crown and Parliament inherited from the medieval and early modern periods by strengthening the hand of civil society. At the same time, the long

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struggle against revolutionary and Napoleonic France gave birth to a new kind of British nationalism and also mobilized bourgeois taxpayers around a program of political and efficiency-oriented administrative reform that eventually resulted in an end to "Old Corruption" and led to the Reform Bill of 1832 (p. 226). The large and powerful trade-union movement called forth by advancing industrialization then collaborated with middle-class reformers to put pressure on the parties for the incorporation of the "respectable" working class into the political community. Tories and Liberals, following the logic of party competition, responded with the 1867 and 1884 Reform Bills, which brought Britain to the threshold of full democracy (pp. 537–38).

While the upheavals of the late eighteenth and the nineteenth century served to solidify tendencies already deeply rooted within Britain, they provoked a substantial break with the past in France. Thus the Revolution not only gave birth to a new kind of French nationalism, but it also led to the elimination of an old regime administration based on the sale of offices and to its replacement with a modern bureaucratic infrastructure. Throughout much of the century, the bourgeoisie struggled against monarchists, Bonapartists, and the labor movement to realize its republican vision, and after 1870 its time finally came. From 1875 the Third Republic was built upon the foundations of universal manhood suffrage (pp. 207–8, 580, 668). Hence though they had taken different routes to reach this point, both Britain and France entered the twentieth century as democratic, bureaucratized, advanced industrial states possessed of substantial military capacities clearly subordinated to civilian authority.

That advanced industrial society need not assume the democratic form found in Britain and France is underlined by the case of imperial Germany, which Mann characterizes as "a new form of modern society . . . capitalist, militarist and semi-authoritarian" (p. 325) and "an alternative route towards modern industrialism" (p. 326). In his view, nineteenth-century Germany underwent a dual process of incorporation: of the territorial states into a new nation and of the new industrial bourgeoisie into the old militarist-bureaucratic state elite. This cooptation of the bourgeoisie in turn drove the German working class, for lack of a potential liberal partner of the kind found in Britain and France, into the arms of Marxist socialism. And that reinforced the bond between bourgeoisie, army, and bureaucracy, all fearful of revolutionary upheaval (pp. 307, 318–26).

The general character of Mann's model of nineteenth- and early-twentieth-century European politics resembles more than any other

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that of Stein Rokkan, whose work is cited approvingly several times. Both authors see the fate of any given state during the era of mass politics as shaped by the cumulative and differential impact of a series of common, formative historical experiences (the Reformation, the national and industrial revolutions), all of which unfolded within a general context of intense geopolitical competition. In this vision, the path followed by each Western European country was at once historically unique, yet shaped by the same general forces of class conflict, nation building and nationalism, and diplomatic and military rivalry that are best understood with the help of social science theory.

This particular way of bringing together the social sciences and history is compelling, but it poses special problems for any attempt to account for variations in the durability of democracies during the interwar years—as Rokkan's efforts show. In one of his last essays, following an exposition of the latest version of his "geoeconomic-geopolitical" model of European development, Rokkan asks whether this model can help explain why democracy collapsed in Italy, Germany, Austria, Spain, and Portugal during the 1920s and 1930s but not in the other countries of Western Europe. In his words: "How can we account for these differences in the mobilization processes between these five and the others? How can we identify the prerequisites for success and the conditions leading to failure in the struggle to maintain competitive pluralism under full-suffrage mobilization?"[9]

Despite his best efforts, Rokkan was not in fact able to provide a parsimonious answer to these questions. As he is forced to admit, "One conclusion is clear; our five cases fall into several distinctive cells on the map; they do not form one single cluster."[10] And later, "The five cases clearly differ in their territory-building histories: early center-building but arrested national integration in Austria and in Spain, early nation-building but frustrated empire-building in the case of Portugal, late center-building with highly homogeneous territories in Germany and Italy."[11] The five cases of democratic failure also differ among themselves on a range of other historical variables employed by Rokkan, and the same is even more true of the even larger and more disparate group of states in which democracy survived the interwar years. Perhaps

[9] Rokkan, "Territories, Nations, Parties: Toward a Geoeconomic-Geopolitical Model for the Explanation of Variation within Western Europe," in Richard Merritt and Bruce Russett, eds., *From National Development to Global Community: Essays in Honor of Karl W. Deutsch* (London: George Allen and Unwin, 1981), 88.

[10] Ibid.

[11] Ibid., 91.

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Mann, in his next volume, will succeed in identifying a limited number of discrete variables capable of explaining why the fate of Britain, France, and many of their neighbors was so different from that of Germany during this period. But it seems more likely that he will instead further deepen his sociologically informed account of the historically unique path pursued by each of these powers through 1945.

While satisfying in its richness of detail and analytic insight, such an account is troubling from a political science perspective both because it is not parsimonious and because in its historical particularity it could imply that Western Europe's experience of democratization is of limited or no relevance to that of other regions. Before accepting this conclusion, however, it is worth considering the alternatives offered by Rueschemeyer, Stephens, and Stephens, Berins Collier and Mahoney, and Luebbert, all of whom emulate Rokkan's methodologically preferable practice of examining all or nearly all countries in a given universe of cases while at the same time attempting to avoid excessively particularistic explanations.

II. Rueschemeyer, Stephens, and Stephens on Democratization

Though the fate of democracy in Europe before 1939 occupies a central position within Rueschemeyer, Stephens, and Stephens's dense and wide-ranging book, their ambitions extend far beyond the bounds of the old continent. Their goal is to clarify the relationship between levels of economic (or, as they put it, "capitalist") development and democracy more generally, by bringing together the often mutually antagonistic cross-national quantitative and historical-comparative literature on this subject. Thus the book begins with the claim that the statistical work of Lipset, Cutright, and others has definitely established that a positive correlation exists between development and democracy, a claim further substantiated by Przeworski and Limongi's recent findings mentioned above.

It is the general causal mechanism underlying this positive correlation that Rueschemeyer, Stephens, and Stephens set out to uncover. Their starting point is that classic of historical-comparative research, Barrington Moore's *Social Origins of Dictatorship and Democracy*.[12] The authors construct a hypothetical model of the relationship between de-

[12] Barrington Moore, Jr., *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston: Beacon Press, 1966).

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velopment and various political regimes based on Moore's arguments as modified by the sympathetic criticisms of Theda Skocpol[13] and their own insights. Taking these modifications of Moore into account, Rueschemeyer, Stephens, and Stephens hypothesize that an increasing level of economic development will favor the advent of "democracy," by which they mean a political regime in which a full array of civil rights is guaranteed and the executive is responsible to a legislature elected by at a minimum all adult males using the secret ballot. Economic development favors democracy, they argue, because it tends to weaken the economic, and hence political, position of antidemocratic large landowners and strengthen that of the prodemocratic working and middle classes. In addition, a rising level of per capita income is associated with the growth of civil society, which acts as a crucial counterweight to authoritarian tendencies and forces within the state. These trends can be accelerated by mass mobilization for war and/or a military defeat, which further strengthens organized labor and weakens old elites. Alternatively, they can be retarded by strong ties of economic dependency and/or the presence of a strong, autonomous state apparatus, both of which tend to buttress the position of old elites and weaken that of the working class.

How can their explanation for the positive relationship between level of economic development and democracy be adequately tested? Rueschemeyer, Stephens, and Stephens, in one of their most significant methodological innovations, do so by examining the extent to which their schema can account for the pattern of dictatorship and democracy found among three large groups of polities from the nineteenth century to the present: all South American states; all Central American and English-speaking Caribbean states; and the advanced industrial states of Western Europe and Britain's white settler colonies (the United States, Canada, Australia, and New Zealand). They justify this procedure by arguing that each of the three groups contains both a significant number of cases and a substantial number of democratic and nondemocratic regimes, a characteristic that sets them apart from other regions such as East Asia and Africa, where nondemocratic regimes predominate. Taken together, the three groups encompass some thirty-eight cases, fewer than those often analyzed in cross-national quantitative studies, but far more than are normally scrutinized in comparative historical works. They thereby avoid the "too many cases–too few variables" problem that so often plagues qualitative research.

[13] Skocpol, "A Critical Review of Barrington Moore's *Social Origins of Dictatorship and Democracy*," in Skocpol, *Social Revolutions in the Modern World* (Cambridge: Cambridge University Press, 1994).

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A crucial test for Rueschemeyer, Stephens, and Stephens, and one at the heart of our own concerns, is the ability of their model to account for (1) the advent of democracy in Western Europe between about 1848 and 1921, (2) its successful consolidation during the troubled 1920s and 1930s in eight states of that region (Britain, France, Switzerland, Belgium, the Netherlands, Norway, Denmark, and Sweden), and (3) its reversal in four others (Italy, Spain, Germany, and Austria).[14] As mentioned above, the authors—in contrast to Marx and Moore—hypothesize that it was the working class rather than the bourgeoisie which was the primary force responsible for the democratic breakthrough both across Western Europe and in other areas of the world as well. However, as they move on to a more detailed examination of their twelve European cases, a rather different picture begins to emerge.

Thus according to Rueschemeyer, Stephens, and Stephens's own account, the democratic breakthroughs in Switzerland (1848) and France (1877) were largely the work of multiclass movements that included artisans and professionals as well as "the bourgeoisie"[15] and, in the Swiss case, a significant body of smallholding farmers. Armed conflict in both instances also helped determine the timing of political change, with Catholic defeat in the Sonderbund civil war of 1847 ushering in the democratic Swiss constitution of 1848 and France's defeat at Sedan in 1870 leading to the overthrow of Louis Napoleon III and the advent of the Third Republic.

Concerning Italy, Rueschemeyer, Stephens, and Stephens state that the working class "played an important, but not leading role in the introduction of democracy," but they then concede that "Giolitti extended suffrage to all adult males in 1912 in an attempt to gain support for his government's Libyan venture" (p. 104). It was the Liberals, "based in the provincial urban upper middle classes," who first introduced universal suffrage into Spain's parliamentary monarchy in 1890, and the transition to democracy in 1931 following the Primo de Rivera dictatorship (1923–31) was "rather like the same process in Italy, as the working class forces were the beneficiary of the introduction of democracy more than the initiator of it" (pp. 120–21).

In Britain it was likewise "middle-class-based (and largely upper-class-led) parties [which] unilaterally extend[ed] effective suffrage to substantial sections of the working class" through the Reform Bills of

[14] Rueschemeyer, Stephens, and Stephens do not include Portugal in their analysis because they do not consider it ever to have experienced full democracy during the interwar period (p. 153).

[15] In a somewhat confusing way, the authors apply the term "bourgeoisie" only to industrial capitalists, using "middle classes" for white-collar employees, professionals, shopkeepers, and other groups that fall between the landed elite on the one hand and workers and peasants/farmers on the other.

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1867 and 1884 (p. 96). At best, the authors can argue that such reforms were "a delayed response to earlier working-class agitation" (p. 96) and that the 1918 act, which established universal and equal male (and unequal female) suffrage in the United Kingdom, was the result of "Labour-Liberal co-operation," though once again war was a precipitating factor in this development. Similarly in Sweden " [i]t was the Liberals, who were based in the urban middle classes, dissenting religions and in small farmers in the north and west, who joined the Social Democrats in the push for suffrage extension" (p. 93, though it was only Germany's defeat in 1918 that finally forced conservatives to grant full parliamentary government.

More promisingly, Rueschemeyer, Stephens, and Stephens assert that in Norway and Denmark the working class "played some role in the drive for democracy" (p. 91). Even there, however, working-class parties and organizations took the lead only in the final push for universal suffrage (introduced into both countries in 1915); earlier steps in the process of democratization were in Norway "largely the work of the peasantry, with the help of sections of the urban middle class," and in Denmark involved an alliance of "the working class, small and medium farmer, and urban middle-class segments" (p. 91). Likewise, to push through equal manhood suffrage in Belgium (1919) and in the Netherlands (1917), socialist workers required the support of Catholics, liberals, and, in the Dutch case, fundamentalist Protestants, as well as the pressures emanating from World War I. In fact, by the authors' own account, it was in Germany and Austria alone that the working class functioned as a true vanguard of democracy through the consistent and solitary support of their respective social democratic parties for parliamentary government and universal suffrage. Only after their antidemocratic opponents had been decisively weakened through defeat in World War I, however, did these parties succeed in transforming the two countries into (short-lived) democratic republics.

III. Berins Collier and Mahoney's Critique

Thus, based on the evidence from twelve Western European cases that they themselves provide, it seems hard to accept Rueschemeyer, Stephens, and Stephens's statement that "[o]ur overview of the transition to democracy confirmed Therborn's . . . contention that the working class . . . was the single most important force in the majority of countries in the final push for universal male suffrage and responsible

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government" (p. 140).[16] My skepticism about the persuasiveness of their argument is shared, moreover, by Ruth Berins Collier and James Mahoney, who conducted their own independent analysis of the first-wave democratization process in eight of the twelve European and three of the eleven Latin American cases studied by Rueschemeyer, Stephens, and Stephens.[17] As Berins Collier and Mahoney put it, "The hypothesis that the working class was central in early transitions to democracy is not supported by the first wave cases" (p. 5). They say, further, that

> as a general proposition . . . an elite project was more important than working class demands as the source of democratization in the first wave. Indeed, in most cases the story of democratization could be told with scant reference to the working class, which either played no role . . . or entered the story primarily as the target of elite mobilization. (p. 62)

In their detailed reexamination of eight European first-wave cases (Britain, France, Switzerland, Norway, Denmark, Sweden, Italy, and Spain), Berins Collier and Mahoney confirm the impression derived from Rueschemeyer, Stephens, and Stephens's evidence (as opposed to their programmatic or conclusory statements) that the working class played at best a collaborative role in the breakthrough to democracy in Norway, Denmark, and Sweden and little direct role in France (at least in 1877) or Switzerland. They also suggest (albeit in my view less persuasively) that the working class was largely indifferent to the English franchise reforms of 1867 and that it was deeply divided over, if not actively hostile to, moves toward democratization in Giolitti's Italy and Primo de Rivera's Spain.

[16] Nor is this claim made more plausible by taking into account the four "advanced capitalist countries" from outside Europe also examined by the authors (the U.S., Canada, Australia, and New Zealand), for their discussion of these cases mentions no working-class role in democratization except in Australia. Their subsequent analysis of evidence drawn from other areas of the world forces Rueschemeyer, Stephens, and Stephens to retreat ever further from their initial desire to assign the working class a vanguard role in first- and second-wave democratization. Thus they admit (p. 181) that "[i]f one analyzes the class forces behind successful and failed attempts to install democratic regimes [in Latin America], the middle classes emerge as the crucial forces behind the alliances effecting initial breakthroughs to restricted democracy and, in collaboration with the working class, to full democracy" (p.181). And the authors are forced to explain the "democratic exceptionalism" of Costa Rica and the English-speaking Caribbean, not by working-class strength or activism, but rather by, respectively, the presence of a prosperous agrarian middle class (p. 259) and the positive legacy of British colonialism, which allowed the emergence of a robust civil society (pp. 265–66, 281).

The Therborn work referred to in the quote in the text is Göran Therborn, "The Rule of Capital and the Rise of Democracy," *New Left Review* 103 (May–June 1977).

[17] I would like to thank John Zysman and David Collier for bringing this very significant working paper to my attention. Ruth Berins Collier has now developed the argument further in her forthcoming book, *Between Elite Negotiation and Working-Class Triumph: Labor and Democratization in Western Europe and South America.*

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If the role of the organized working class in bringing about first-wave democratization was as limited as these authors claim, then one might reasonably ask why it has attracted so much scholarly attention, not only in the contributions of Therborn and Rueschemeyer, Stephens, and Stephens, but also in many works of social history. Berins Collier and Mahoney themselves provide a very plausible answer to this question. They argue that because the predecessor regimes to Europe's first-wave democracies were generally liberal oligarchies in which voting rights were limited through property requirements, commentators at the time and subsequently conceived of democratization during this period as "the class-defined extension of participation" (p. 63). It was, they believed, a process in which those groups standing outside the polity—generally workers, the petite bourgeoisie, and sometimes farmers/peasants—sought to wrest the right of admission from those already enfranchised, namely, the landed upper class, capitalists, and some middle-class groups. Since the workers as a whole had the most to gain from full democratization, it seemed logical to assume that they would be the driving force behind this process. The problem is that such an assumption both overestimates the size of the working class and the strength of its organizations prior to 1918 and underestimates the ambivalence and even hostility of those organizations to what anarcho-syndicalists, many socialists, and even some trade union activists saw as "bourgeois democracy."

Berins Collier and Mahoney do more, however, than simply refute the hypothesis that it was principally the working class which was responsible for the breakthrough to democracy in Western Europe. They also offer an alternative view of that process, one that identifies four distinct patterns of first-wave democratization both on the old continent and in Latin America. In the first of these, which they call "pre-labor democratization" (Switzerland 1848, Denmark 1848, as well as Chile 1874).

> labor played no role in the establishment of a democratic regime due to the simple fact that the transition to democracy occurred prior to the development of a significant working class. Here democracy was the product of an intra-elite struggle. . . . This pattern reveals that in no sense can one say that first-wave democratization necessarily required working class pressures or in fact any working class role whatsover. (p. 8)

Thus the democratic Swiss constitution of 1848 was the direct result of the civil war of 1847 in which religion and region, not class, were the principal lines of cleavage; and the overthrow of Danish absolutism and the introduction of universal suffrage in 1848 occurred in the wake of the 1848 revolution in a country that was still almost entirely agrarian.

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In the second pattern, "electoral support mobilization" (England 1867, 1884, Italy 1912, as well as Uruguay 1919),

> the enfranchisement of the working class can be understood as a strategy of political entrepreneurship to mobilize a larger support base in a context of political competition. Here democratization was an elite project, and the working class was the recipient rather than the initiator of democracy. (p. 11)

The best-known example of this was Disraeli's expansion of the suffrage in 1867 in order to capture newly enfranchised voters for the Conservatives, followed in 1884 by Gladstone's attempt to do the same for the Liberals.

In the third pattern, "middle sector democratization" (France 1875, Spain 1931, as well as Argentina 1912), democracy was "a product of the efforts of middle-sector groups. . . . Political reform was thus brought about as a defensive response to middle-sector pressures for inclusion, which, in taking the form of democracy, also included the working class" (p. 18). Prominent examples of this pattern include the decision to establish a democratic, republican regime in France following the defeat of 1871 and the replacement of the Spanish monarchy with a republic in 1931, both in the wake of largely middle-class mobilization. Finally, in Norway (1898), Denmark (1915), and Sweden (1918), democratization was a "joint project" in which "the working class had already been partially enfranchised and labor-affiliated parties managed to extend the franchise to the point of full manhood or universal suffrage" (p. 25).

Illuminating as this typology is for understanding democratic breakthroughs in nineteenth- and early-twentieth-century Europe, it still begs the question of why one road to democracy rather than another was taken by a given country. Berins Collier and Mahoney do not really provide an answer, but they succeed in refuting in a most convincing way the notion that one class or fixed combination of classes (Marx and Moore: the bourgeoisie; Therborn and Rueschemeyer, Stephens, and Stephens: the working class; many modernization theorists: the middle classes) can claim to be the principal carrier of democracy. Furthermore, Berins Collier and Mahoney's identification of four disparate patterns of first-wave democratization in Western Europe (and Latin America) accords well with Przeworski and Limongi's findings based on post–World War II cases, that democracies can come into being in many different ways and for many different reasons.[18]

[18] Przeworski and Limongi (fn. 2), 158.

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IV. Rueschemeyer, Stephens, and Stephens on Democratic Breakdown and Survival

Once democracy had arrived—under whatever circumstances—in Western Europe, why did it prove irreversible in some countries but not in others during the interwar period? In answering this question (not addressed by Berins Collier and Mahoney), Rueschemeyer, Stephens, and Stephens argue, with Moore, that it was the absence of a politically powerful landed elite which permitted democracy to survive in eight Western European states during the 1920s and 1930s. Conversely, it was the presence of such an elite, allied as it was with antidemocratic elements within the state apparatus, that was responsible for fascist triumphs in interwar Europe and Japan. Aside from the problematic case of Britain, where a strong landed elite coexisted with stable democracy, the Moore thesis seems at first glance to hold up well when extended beyond the small number of countries examined in *Social Origins*, since the presence of a powerful group of large landowners in Italy, Spain, and Germany did in fact go hand in hand with democratic breakdown. Yet, as with their discussion of the advent of democracy, Rueschemeyer, Stephens, and Stephens's detailed examination of these three authoritarian cases fails to provide convincing support for their general argument.[19]

In Italy, for example, the authors concede that "the landed upper class did not assume the political leadership of the country, thus Italy does not fit Moore's pattern of a landholder-dominated state in alliance with a dependent bourgeoisie" (p. 103). In addition, they admit that, in the land of Gramsci, "upper-class ideological hegemony over other groups is less important in accounting for the outcome than in the German or Austrian cases" (p. 105). While Rueschemeyer, Stephens, and Stephens can correctly underline the important role played by Po Valley landlords in supporting the Fascists, they also allow that the movement drew support from across the class spectrum, at least in the North. Furthermore, the sympathy that the Fascists enjoyed within the security forces was crucial in permitting them to come to power.

The authors also point to interesting parallels between Italy and Spain—none of which, however, help support the Moore thesis. Thus they state that "as in the Italian case, the initial development of [Spain's] ruling political coalition in the nineteenth century does not

[19] This is even more true of their discussion of the Austrian case, a detailed evaluation of which has not been included here because it is not, in my view, strictly comparable to the instances of interwar breakdown among those Western European states that already existed before 1918 (see fn. 7). In several places on page 118, the authors admit the extent to which Austria presents problems for their general argument.

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seem to point in the direction of the landlord-bourgeois-state alliance as responsible for the development of modern authoritarianism" (p. 119). Further, "[a]s in Italy, it cannot be said that the bourgeoisie was dependent on a landholder-dominated state." (p. 120). And finally, "given the low level of capitalist development of the country and thus the weakness of civil society, ideological hegemony of the authoritarian (upper class) forces did not play such a crucial role in the Spanish case" (p. 121). Rather, it was the army that was primarily responsible for the overthrow and defeat of the Spanish Second Republic; it had a long tradition of political interventions throughout the nineteenth century, and most of its officers were, as the authors note, of humble origin.

Even the case of Germany—in which landed elites in the person of the Junkers seem to have played the most clear-cut role both in hindering the transition to democracy before 1918 and in weakening the Weimar Republic thereafter—is no longer considered to fit the Moore paradigm as well as it once did. As Rueschemeyer, Stephens, and Stephens point out, the work of David Blackbourn, Geoff Eley, and David Calleo, which has gained wide scholarly acceptance, argues that "the dominant view which locates the cause of German authoritarianism and imperialism in the political dominance of the Junkers and the politically dependent, indeed 'supine' posture of the bourgeoisie is flawed" (p. 106). While it may well be true that the Junkers exercised a significant degree of political influence during the Kaiserreich both directly and indirectly through their positions within the army and bureaucracy, "the role of the agrarian upper classes and the authoritarian coalition more generally in the breakdown of Weimar democracy is . . . a point of contention" (p. 109).

As Rueschemeyer, Stephens, and Stephens note, the mass electoral and membership base for the Nazis was furnished not by the Junkers, who even in the eastern provinces owned only 40.5 percent of the agricultural land,[20] nor by the peasants living in these sparsely populated provinces. Rather, it was provided by the Protestant middle strata and peasant smallholders of central and western Germany. Here the authors attempt to save the Moore thesis by arguing (without much evidence) that "[i]n part through deliberate political campaigns, the dominant classes [a much broader concept than "landed elites"—T.E.] developed an ideological hegemony over significant sections of the Protestant peasantry and urban middle classes" (p. 115). This claim runs very much counter to current historical research on Germany, which rejects

[20] Alan Milward and S. B. Saul, *The Development of the Economies of Continental Europe, 1850–1914* (Cambridge: Harvard University Press, 1977), 57.

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the notion that the wide prevalence of antidemocratic thinking and movements during both the imperial and the Weimar periods can be seen simply as the result of "manipulation" from above on the part of either a "ruling elite" more generally or the Junkers more specifically.[21]

Given the great difficulties they exhibit in establishing a decisive causal connection between either direct or "lagged" political interventions on the part of a landed elite and the collapse of democracy in Italy, Spain, and even Germany, it is difficult to agree with Herbert Kitschelt's assessment that "[c]onsistent with Moore, Rueschemeyer, Stephens, and Stephens show that labor-intensive and labor-repressive agriculture under the direction of large landowners is inimical to democracy"; and that "in the spirit of Moore, there is one very important hard-and-fast message that consistently comes across in Rueschemeyer, Stephens, and Stephens's study: countries with labor-intensive and labor-repressive agriculture will not become stable democracies."[22] Furthermore, even if it were true that the mere presence of an antidemocratic landed elite in a given Western European country was sufficient to undermine democracy, this would not necessarily mean, as the authors imply, that the mere absence of such an elite would alone be enough to secure democracy's future. They, along with many other analysts, seem to assume that the survival of democratic systems in interwar Western Europe was the expected outcome and that their collapse was the deviant result most in need of explanation.

Rather than buttress the Moore thesis, then, Rueschemeyer, Stephens, and Stephens have, thanks to their methodological innovations and scrupulous presentation of the evidence, demonstrated something quite different: that the process of democratic transition, survival, and breakdown in Europe (and in the rest of the world as well) is too complex to be accounted for adequately within the classical frameworks offered by Marx or Moore. Yet while they perhaps unwittingly provide sufficient evidence to undermine older analytic frameworks, they are not able to replace them with anything equally parsimonious. As Kitschelt also notes,[23] the authors are instead driven repeatedly to introduce new causal factors (war, divergent state-building legacies, level of institutionalization of right-wing political parties, strength of civil society, religion, the impact of British colonialism) in order to do the

[21] David Blackbourn, *The Fontana History of Germany, 1780–1918: The Long Nineteenth Century* (London: Fontana, 1997), 426–32; James Retallack, *Germany in the Age of Kaiser Wilhelm II* (London: Macmillan, 1996), 51.

[22] Kitschelt, "Political Regime Change: Structure and Process-Driven Explanations?" *American Political Science Review* 86 (December 1992), 1030, 1031.

[23] Ibid., 1031.

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historical record justice. Might Gregory Luebbert's *Liberalism, Fascism or Social Democracy* offer a more convincing explanation of democratic durability and failure in Western Europe before 1939?

V. LUEBBERT

Luebbert's ambitions are, perhaps wisely, more limited than those of Rueschemeyer, Stephens, and Stephens. Rather than seeking to identify the origins of dictatorship and democracy across a number of disparate regions, he concentrates on explaining the outcomes found in interwar Europe. In contrast to Rueschemeyer, Stephens, and Stephens, Luebbert identifies four rather than just two such outcomes: liberal democracy (democratic polity plus orthodox economic policies); social democracy (democratic polity plus stimulatory economic policies); fascism (totalitarian polity and corporatist economic policies); and traditional dictatorship (greater toleration of dissent and a lower level of mobilization than in fascism).

Only in passing does Luebbert discuss the origins of traditional dictatorships, which in his view were confined during this period entirely to East-Central and Eastern Europe. Instead, he focuses nearly all of his efforts on accounting for the distribution of the other three regime types across the western portion of the Continent. Like Rueschemeyer, Stephens, and Stephens, he remains unconvinced by Moore's argument about the insignificance of small countries and hence includes in his analysis nearly the entire universe of Western European cases with the exception of Portugal.[24]

For Luebbert, the claim made by Moore and by Rueschemeyer, Stephens, and Stephens—that it was the presence or absence of a powerful landed elite dependent on a friendly state that determined whether democracy failed or survived during the interwar years—is empirically flawed. In two short but hard-hitting paragraphs in the conclusion (pp. 308–9), he asserts that no correlation exists between the size of the dependent agricultural labor force (and hence the degree of landlords' dependence on the state to protect them against their agricultural workers) and regime outcome. Second, he stresses that economic control on the part of landed elites did not necessarily mean control over voting behavior, citing in support the tendency of agricultural laborers in southern Spain to vote socialist. And finally, he argues

[24] Like Rueschemeyer, Stephens, and Stephens, Luebbert makes no mention of Portugal. As stated earlier, he briefly considers the Austrian case (pp. 263–65)—rightly in my view—together with those of other European states created after 1918 in the wake of the collapse of the great empires.

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that support for fascism among peasants was strongest precisely in those areas *not* dominated by large landowners, such as north-central Germany and northern Spain and Italy.

Not only does Luebbert think that landlords were not crucial in bringing about fascist outcomes, but he also believes that, as a result of their small numbers, they became relatively insignificant for politics more generally once Europe had entered the age of mass mobilization and mass parties. Where he does agree with Moore, as well as with Rueschemeyer, Stephens, and Stephens, is in viewing classes as fundamental political actors and class coalitions as the structural underpinnings of particular regime types. However, Luebbert does not agree with the authors we have discussed that classes should be employed as the sole unit of political analysis, since, as he points out, classes ultimately must act through parties. Hence it is to party configurations and behavior that he first looks to explain why certain liberal democratic regimes in the Europe of the 1920s and 1930s successfully withstood the crises of the interwar years and why others gave way to social democracy or fascism.

Luebbert's sometimes rather convoluted argument can be summarized as follows. In three Western European countries, Britain, France, and Switzerland, liberal ideas and liberal parties were politically hegemonic prior to World War I. This hegemony was rooted both in the fact that "[the liberal parties'] most natural potential constituencies, the middle classes, were not politically divided by antagonisms within them rooted in religious, regional, linguistic and urban-rural differences" (p. 7) and in the fact that, given this secure electoral base, liberals were able to make concessions which permitted them to ally with the emerging workers' movement and, later, nascent workers' parties. This prewar "lib-labism" brought forth organizationally weak trade unions and workers' parties that largely accepted the liberal-created socioeconomic order and were unwilling or unable to challenge it during the crisis-ridden interwar years. Stability was assured in these three countries during the 1920s by "the formation of center-right coalitions of middle-class consolidation that left socialist parties isolated and ineffective" and that held to conservative economic policies throughout the Great Depression (p. 8).

By contrast, those countries that had not experienced "liberal hegemony" prior to 1918 could not, in Luebbert's view, become stable liberal democracies during the interwar years, but rather faced the choice of fascism or social democracy. These "aliberal" cases include Norway, Sweden, and Denmark, as well as Germany, Italy, and Spain. In none of

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these states, according to Luebbert, were liberal parties able to mobilize sufficient support to transform themselves into dominant parties of government in the decades prior to World War I; as a result such parties were unable to form effective lib-lab alliances with workers' groups. Hence the workers' parties that emerged and gathered strength in these countries during the late nineteenth and the early twentieth century were not moderate and accommodationist in character but rather were strong, organizationally self-contained parties modeled on the German SPD.

For Luebbert, the key question in aliberal societies during the interwar years was whether it would be possible for social democrats or socialists to make common cause with parties representing other classes in order to create durable, democratic governments capable of stabilizing the economy through corporatist interventions in the labor market. In Scandinavia workers found such allies in the form of farmers' parties, and the result was the emergence during the 1930s of social democratic regimes that have remained hegemonic to this day in Sweden, Norway, and, to a lesser extent, Denmark. In Germany, Italy, and Spain, however, parties of the democratic left were unable to find effective coalition partners because farmers in those countries sided with the urban middle classes rather than with workers, thereby providing a mass base for fascist regimes, the aim of which was to smash the workers' movement by force. Luebbert explains this critical difference in the behavior of farmers—alliance with workers' parties in the social democratic cases, repudiation of those parties and support for the far right in the fascist cases—by pointing to differences in the level of success enjoyed by socialist parties in organizing farm workers in the two groups of countries. In Germany, Italy, and Spain, he argues, such parties and their union affiliates were able to enroll a significant number of agricultural laborers after 1918, thereby alienating their farmer-employers, whereas in Scandinavia socialists had far less success in this area because most farm workers had already been "captured" by other parties. This relative failure then left the door open to the future worker-farmer alliance that would emerge in the 1930s.

How convincing is Luebbert's parsimonious, highly original argument? A first objection, already made by Kitschelt, is that two of the eleven cases he discusses do not seem to fit his argument.[25] Thus Luebbert classifies the Netherlands and Belgium as states in which liberal parties were not hegemonic prior to 1914 and hence states where liblabism did not develop. As a result, they should have become social de-

[25] Kitschelt (fn. 22), 1029.

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mocracies during the 1930s but did not do so. In fact, religious parties (Catholic in Belgium and both Catholic and Calvinist in the Netherlands) dominated the governments of both countries during the interwar period and held fast to classically liberal economic policies in response to the depression.

Yet even if we consider these two cases to be anomalous and focus instead on the remaining nine countries, other difficulties appear. The core of Luebbert's argument can be reduced to two claims: that the presence or absence of "liberal hegemony" prior to 1914 determined whether a given Western European country would become a liberal democracy during the interwar period or would be condemned to a choice between social democracy or fascism; and that this latter choice was itself determined by whether the country's socialist/social democratic party became "ensnared in rural class conflict" by attempting to organize rural workers (fascism) or desisting from doing so (social democracy). The problem with the liberal hegemony argument is that this hegemony seems extremely difficult to measure independent of its supposed interwar consequences. Thus, in both Denmark and Norway, the Venstre (Left), a liberal party that attracted support from the middle classes, workers, and farmers, was the leading political force before 1914, yet Luebbert refuses to categorize these states as "liberal hegemonic." At the same time France, a country without a single dominant lib-lab party, earns this designation.

Meanwhile, the contention that it was the degree of success enjoyed by socialist/social democratic parties in organizing agricultural workers that determined the willingness of family farmers to enter into coalitions with such parties, while clever and original, seems to lack any strong basis in fact. Surely it was not the numbers of agricultural workers enrolled in socialist unions but the effort to organize such workers at all—whatever the ultimate success of the endeavor—that would have antagonized family farmers most, and the member parties of the Socialist International engaged in such efforts from the 1890s onward.[26] Furthermore, given the deep hostility of Protestant (and Catholic) farmers in Germany toward the social democrats as far back as the 1890s, it defies belief to assert that the possibility of a red-green alliance in Germany was precluded only by sudden SPD successes in organizing farm laborers after 1918, as Luebbert claims (pp. 298–300).

Where does this leave us? Luebbert's underlying contention is that democracy survived during the interwar period in eight Western Euro-

[26] Hans Georg Lehmann, *Die Agrarfrage in der Theorie und Praxis der deutschen und internationalen Sozialdemokratie* (Tübingen: J. C. B. Mohr, 1970), 48, 267–69.

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pean states because, during the crisis-filled years after 1929, either the middle classes gave their votes to prodemocratic parties or workers and farmers were able to cooperate to defend the political system, whereas it failed in three others because the middle classes and farmers threw their support behind antidemocratic forces. This contention may be true, but it merely begs the question as to why parties and voters acted in the way they did. Luebbert's own attempt to answer this question by pointing to divergent patterns of working-class (including *rural* working-class) incorporation before 1914 is highly original, but in my view unconvincing. Is there an alternative?

VI. An Alternative Approach: Associational Life, Parties, and Party-Center Politics

I believe there is an alternative, and it is one that builds on the achievements of the literature just discussed by examining variations in interwar outcome across the full range of Western European cases, just as Rueschemeyer, Stephens, and Stephens, and Luebbert (and before them Rokkan) did. Furthermore, Luebbert's concentration on the ability of prodemocratic parties to hold on to their supporters and to build governing coalitions with rival parties committed to democracy seems a more plausible first-order explanation for democratic durability under the unfavorable conditions of the interwar years than either the class-coalitional approach of Rueschemeyer, Stephens, and Stephens or the historical particularism of Mann, despite the other merits of these works. I would suggest, however, that rather than focusing exclusively on working-class incorporation before 1914 as the key independent variable in accounting for interwar party strength and behavior—as Luebbert does—we "bring civil society back in" and examine instead the relationship between associational life and political parties in our twelve cases during that time period.

It has long been recognized that most Western European states of the late nineteenth and early twentieth centuries possessed, in common with the United States and Britain's white dominions, highly developed associational landscapes and that, if Tocqueville is to be believed, such landscapes played a significant role in defending liberty against the potential for despotism lurking within modern society. They accomplished this both by furthering participation, trust, and civic virtue among citizens and by acting as a potential counterweight to an ever more powerful state. The overshadowing of associational life by vertical paron-client ties before 1914 across large areas of rural Spain and Por-

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tugal—countries that would later experience democratic breakdowns—lends prima facie plausibility to the idea that a direct correlation might exist between the strength of associational life and the durability of democracy.

However, the deviant cases of Germany and northern Italy forcefully contradict the hope that variations in the strength of civil society alone might explain the pattern of outcomes found across interwar Western Europe during the 1920s and 1930s. Germany possessed one of the world's densest associational landscapes before 1933 and yet national socialism won a substantial popular following there. Moreover, recent scholarship on Weimar Germany has detailed the crucial role that local associational networks played in promoting the rapid spread of support for the Nazis, thereby permitting the latter's spectacular electoral gains of September 1930 and July 1932.[27] In Italy it was precisely the two areas identified by Robert Putnam as possessing the richest associational life between 1860 and 1920—Emilia-Romagna and Lombardy—that provided the Fascist movement with its earliest and most fervent supporters. Indeed, it was from Bologna, one of Italy's most civic cities, that Fascism radiated outward to Ferrara and the highly associational Po Valley after 1920.[28]

These cases indicate that some other factor or factors combined with divergent patterns of associational life before 1914 to produce the outcomes observed during the interwar period. The search for such a factor or factors was first taken up nearly three decades ago in a number of works, most notably those of Harry Eckstein and Stein Rokkan on Norway and Arend Lijphart on the Netherlands. These works explored the connection between particular kinds of associational landscapes and the durability of democracy, but this analytic approach soon fell out of favor.[29] More recently, however, historians have produced a consider-

[27] Rudy Koshar, *Social Life, Local Politics, and Nazism: Marburg, 1880–1935* (Chapel Hill: University of North Carolina Press, 1986), 179–208, 298–99; idem, "Two 'Nazisms': The Social Context of Nazi Mobilization in Marburg and Tübingen," *Social History* 7 (January 1982); Zdenek Zofka, *Die Ausbreitung des Nationalsozialismus auf dem Lande* (Munich: Kommissionsbuchhandlung R. Wölfle, 1979), 37, 81. Sheri Berman has made a similar argument in Berman, "Civil Society and the Collapse of the Weimar Republic," *World Politics* 49 (April 1997).

[28] Robert Putnam, *Making Democracy Work: Civic Traditions in Modern Italy* (Princeton: Princeton University Press, 1993), 148–51; Paul Corner, *Fascism in Ferrara, 1915–1925* (Oxford: Oxford University Press, 1975), 137–38. Paolo Farnetti also provides figures that indicate that more than 250,000 of the Fascist movement's 332,310 members in May 1922 were found in the North, over 130,000 of them in Lombardy and Emilia alone. See Farnetti, "Social Conflict, Parliamentary Fragmentation, Institutional Shift, and the Rise of Fascism: Italy," in Juan Linz and Alfred Stepan, eds., *The Breakdown of Democratic Regimes: Europe* (Baltimore: Johns Hopkins University Press, 1978), 22.

[29] Arend Lijphart, *The Politics of Accommodation: Pluralism and Democracy in the Netherlands*, 2d ed. (Berkeley: University of California Press, 1975); Harry Eckstein, *Division and Cohesion in Democracy: A Study of Norway* (Princeton: Princeton University Press, 1966); Stein Rokkan, "Geography, Religion

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able number of monographs, often with a local or regional focus, that provide a substantial amount of information on associational life and its ties to politics in all of the Western European states during the late nineteenth and the early twentieth century.[30]

What this literature, both old and new, seems to imply is that political change and the character of civil society in late-nineteenth-century Europe interacted with one another to produce the distinctive patterns in the relationship between political parties and associational life that underlay divergent interwar outcomes. More concretely, where parties and party competition stood at the center of political life before 1914 and the associational landscape was well developed (Britain, France, Scandinavia, Switzerland, Belgium, the Netherlands), the two came to reinforce each other in such a way as to further democratization and increase the durability of the resulting democratic regimes after 1918. Conversely, where the associational landscape was well developed but parties and party competition were not central to political life (Germany and Italy), conservative political forces were fragmented and only weakly tied to bourgeois and agrarian associational networks. This situation created conditions favorable to the sudden success of far-right movements of agrarian and bourgeois defense under the crisis conditions of the interwar period.

The opposite situation, where associational life before 1914 was weak but party government strong (Spain and Portugal), tended to reinforce patron-client networks and the cacique politics associated with them. When more modern right-wing parties emerged after 1918 in response to left-wing parties firmly rooted in associational subcultures,

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and Social Class: Crosscutting Cleavages in Norwegian Politics," in Seymour Martin Lipset and Stein Rokkan, eds., *Party Systems and Voter Alignments: Cross-National Perspectives* (New York: Free Press, 1967). See also a collection of Rokkan's essays, the majority of which are on Norway: Rokkan, *Stat, Nasjon, Klasse* (Oslo: Univesitetsforlaget, 1987).

[30] Examples of such works include Eugenio Biagini and Alistair Reid, eds., *Currents of Radicalism: Popular Radicalism, Organised Labour and Party Politics in Britain, 1850–1914* (Cambridge: Cambridge University Press, 1991); Martin Pugh, *The Tories and the People, 1880–1935* (Oxford: Basil Blackwell, 1985); Raymond Huard, *Le mouvement républicain en Bas-Languedoc* (Paris: Presses de la F.N.S.P., 1982); idem, *La naissance du parti politique en France* (Paris: Presses de la F.N.S.P., 1996); Hans Righart, *De Katholieke Zuil in Europe* (Amsterdam: Boom Meppel, 1986); Jan van Miert, *Wars van Clubgeest en Partijzucht: Liberalen, Natie en Verzuiling, Tiel en Winschoten, 1850–1920* (Amsterdam: Amsterdam University Press, [1994]); Koshar (fn. 27, 1986); Sven Lundkvist, *Folkrörelserna i det Svanska Samhället 1850–1920* (Stockholm: Almquist and Wiksell, 1977); Siegfried Weichlein, *Sozialmilieus und politische Kultur in der Weimarer Republik* (Göttingen: Vandenhoeck und Ruprecht, 1996); Franz Walter, Tobias Dürr, and Klaus Schmidtke, *Die SPD in Sachsen und Thüringen zwischen Hochburg und Diaspora* (Bonn: Verlag J. H. W. Dietz Nachf., 1993); Anthony Cardoza, *Agrarian Elites and Italian Fascism: The Province of Bologna, 1901–1926* (Princeton: Princeton University Press, 1982); Mary Vincent, *Catholicism in the Second Spanish Republic: Religion and Politics in Salamanca, 1930–1936* (Oxford: Clarendon Press, 1996); Pedro Tavares de Almeida, *Eleções e Caciquismo no Portugal Oitocentista, 1868–1890* (Lisbon: Difusão Editorial, 1991).

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they remained weak and divided among themselves, leading their supporters to seek military assistance to counter the threat from the left. Finally, moving beyond the borders of Western Europe, Russia possessed neither party-centered politics nor an extensively developed associational landscape before 1914. The result there was a pattern of very weak parties and conspiratorial organizing that helped make possible the Bolshevik overthrow of the Kerensky government. Perhaps some additional historical detail will render the logic of this argument clearer.

The second half of the nineteenth century saw a tremendous upsurge in Britain, France, Norway, Denmark, and Sweden in the creation of and membership in voluntary organizations: trade unions, cooperatives, agricultural pressure groups, educational associations, temperance groups, and dissenting sects in Britain and Scandinavia; trade unions, agriculture pressure groups, educational associations, reading circles, and Masonic lodges in France.[31] Such organizations helped mobilize citizens, bind them to one another, and involve them in public affairs during a period of massive and disorienting social dislocation. At the same time, parties and party competition in all of these states came to assume central importance in political life. In Britain, France, and Norway this was a direct result of both full parliamentarization (executives responsible solely to a parliamentary majority) and expansion of suffrage; in Denmark and Sweden it was the result of suffrage expansion and an ongoing struggle over parliamentarization that pitted liberal and conservatives forces within the national legislatures against one another.

The number and character of the modern political parties that began to crystallize and erect national networks of offices or committees in these countries between the 1850s and the First World War varied from case to case. In Britain these parties were of course the Liberals, Conservatives, and, after the turn of the century, Labour. In Scandinavia the usual party pattern included the socialists on the left, Venstre (the Left, often rendered in English as the Liberals) on the center left, Conservatives on the right, and, somewhat later, Agrarians in the center.[32] The French party landscape was more fragmented, running from

[31] Christiane Eisenberg, "Arbeiter, Bürger und der 'bürgerliche Verein,' 1820–1870: Deutschland und England im Vergleich," in Jürgen Kocka, ed., *Bürgertum im 19. Jahrhundert: Deutschland im Europäischen Vergleich*, 3 vols. (Munich: Deutscher Taschenbuch Verlag, 1988), 2:195–208; Jean-Marie Mayeur, *La vie politique sous la Troisième République, 1870–1940* (Paris: Éditions du Seuil, 1984), 76–78; Righart (fn. 30), 100–119, 147–68, 209–37; Lundkvist (fn. 30), 66–86; Tim Knudsen, *Den Danske Stat i Europa* (Copenhagen: Jurist- og okonomforbundets Forlag, 1993), 96–99, 151–54; Eckstein (fn. 29), 102–3; Rokkan (fn. 29), 374.

[32] T. K. Derry, *History of Scandinavia* (Minneapolis: University of Minnesota Press, 1979), 257–63, 266–68; Dankwort Rustow, *The Politics of Compromise* (Princeton: Princeton University Press, 1955), 44–54, 56–60, 65–78; Rokkan (fn. 29), 391, 394–95.

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a number of socialist groups on the left through the radicals on the center left, various republican groupings occupying the center ground, and several parties close to the Catholic church on the right.[33]

Finding themselves confronted with diverse and well-organized civil societies, emergent parties in these countries sought to forge ties with associations and win over their members, but the resulting overlap between the associational and party landscapes was far from perfect. Thus, trade unionists in Britain were known to support, albeit in lesser numbers, the Conservatives as well as the Liberals and, later, Labour, and dissenters were known to support both the Liberals and Labour. In Scandinavia farmers split their votes among the Conservatives, the Liberals and, later, the Agrarians; dissenters voted for the Liberals, Social Democrats, and Agrarians; and trade unionists voted for the Liberals and Social Democrats. Finally, in France members of associational groups could chose from at least two political groupings at every point across the political spectrum.[34]

This lack of a one-to-one correspondence between associational groupings and political parties had a beneficial effect on the long-term political trajectory of these nations. On the one hand, it allowed individuals whose views may have differed on many other issues to organize reform campaigns that cut across party and class lines, as happened with the free trade, temperance, and suffrage issues in both Britain and Scandinavia. On the other hand, it forced the parties to remain pragmatic and flexible in their positions in order to win the support of a range of interest groups and react to new currents within civil society.

The cases of Belgium, Switzerland, and the Netherlands indicate, however, that this particular pattern of interaction between a dense associational landscape and powerful political parties was not the only one that could lead to durable democracy during the interwar years. The centrality of party competition came to all of these countries relatively early as a result of full parliamentarization (Belgium 1847; Switzerland 1848; the Netherlands 1868). In all three cases liberal parties that attracted support from participants in a range of associations—from business groups to workers' aid societies to Masonic lodges —enjoyed an initial dominance within government. The liberals' opponents—Catholics and socialists in Belgium; Catholics, socialists, and later farmers in Switzerland; Catholics, socialists, and orthodox Calvinists in the Netherlands—responded to their control of state institutions, and especially of the education system, by creating their own dense associ-

[33] Mayeur (fn. 31), 193–204.
[34] Ibid.

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ational subcultures comprising workers, youth, and leisure time and other organizations in order to protect their supporters from liberal influence and to mobilize them more effectively in the struggle to gain parliamentary seats.[35]

Once they obtained a foothold in government, the antiliberal parties did not attempt to dismantle liberal state institutions. Rather, they sought to obtain public funds for their own schools and, later, welfare funds, which they themselves administered. They also often pressed for a further expansion of the suffrage, since their support base extended further down the social scale than did that of the liberals. By the turn of the century, in the wake of suffrage reform, the Catholics had already replaced the liberals as the dominant force in Belgian politics, while the Catholics and Calvinists were about to do the same in the Netherlands. Both trends continued following the introduction of universal (Netherlands 1920) and equal manhood suffrage (Belgium 1919). Even in Switzerland, long since fully democratic, the hegemonic liberals were forced in 1891 to share power with the Catholic party and, after the introduction of proportional representation in 1919, with the farmers as well. Thanks to the financial backing for their subcultural institutions, which these nonliberal parties received from the state, and to their now-entrenched positions within government, all were firm supporters of the democratic order. Furthermore, because party leaders across the political spectrum could count on the firm support of their voters thanks to the membership of those voters in all-encompassing associational subcultures, they enjoyed the freedom to reach compromises with their opponents that, although in the national interest, may have been naturally distasteful to their own followers.[36]

These three consociational cases stand in striking contrast to the situation in Germany and Italy, where of course fascism was to triumph over democracy during the interwar period. These countries also possessed a well-developed bourgeois associational landscape during the second half of the nineteenth century, but in neither case did a party-centered pattern of politics emerge before 1914. In Germany this was because the extensive executive powers retained by both emperor and chancellor tended to emasculate the Reichstag and, as Max Weber pointed out, encourage talented men to enter the bureaucracy, the army, or business rather than party politics.[37] In Italy, which did possess par-

[35] Righart (fn. 30), 100–119, 146–69, 209–37.
[36] Lijphart (fn. 29), 190–91.
[37] Max Weber, "Parliament und Regierung im neugeordneten Deutschland," in Weber, *Gesammelte Politische Schriften*, 4th ed. (Tübingen: J. C. B. Mohr, 1980), 311–21, 337–50.

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liamentary government, it was the practice of *trasformismo* or the cooptation of individual opposition deputies into the ruling coalition that undermined any semblance of party discipline.[38]

The result in both cases was a pattern of at best weak ties between the bourgeois and agrarian associational milieus and the debilitated, ineffectual liberal and conservative parties, which aspired to be their political representatives. In Germany the participants in the very extensive networks of patriotic, choral, and shooting associations and the economic interest groups cultivated a growing antipathy toward party politicians in favor of a belief in the superiority of government through bureaucratic experts.[39] In Italy *trasformismo* meant that parties on the center-right of the political spectrum barely existed as such. At the same time, the self-interested maneuverings of parliamentary deputies fostered contempt for parliamentarism among an ever wider circle of Italian society.[40]

Outside of the bourgeois and agrarian milieus, however, effective socialist and social Catholic parties did arise, primarily as defensive organizations seeking to protect their supporters against what were seen as antiworker and anticlerical states. As in the consociational cases, these parties—the SPD and Zentrum in Germany and, somewhat later, the PSI and Partito Populare in Italy—built up their own extensive associational subcultures embracing trade unions, youth, leisure-time and woman's organizations, and mutual aid societies. Under the crisis conditions that reigned in both countries after 1918, the compact socialist and Catholic politico-associational subcultures were perceived as particularly threatening by the bourgeois and agrarian milieus, which lacked political parties of comparable strength and cohesion. As a result, association members from these milieus proved particularly susceptible to the appeals of movements aimed at uniting all center-right forces for the purpose of overthrowing corrupt parliamentary states and breaking the power of socialist and clerical parties. As mentioned above, the dense nature of bourgeois and agrarian associational networks in turn permitted support for both Fascism and national socialism to spread extremely rapidly through (economically advanced) northern and central Italy and all of Germany, respectively.

If Germany and Italy represent cases where a well-developed associational landscape in the absence of party-centered politics brought

[38] Christopher Seton-Watson, *Italy from Liberalism to Fascism, 1870–1925* (London: Methuen, 1967), 51–52, 91–92 246–48, 390.

[39] Koshar (fn. 27, 1986), 6, 45–90.

[40] Seton-Watson (fn. 38), 91–92, 390.

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forth results fatal to democracy, the opposite set of conditions produced similar results in Spain and Portugal. Here parliamentary government centered around two parties (Liberals and Liberal-Conservatives in Spain; Regenerators and Progressists in Portugal) was in place by the 1870s at the latest. This party-centered politics arose, however, in societies where patron-client chains rather than voluntary associations were still the prevalent form of social organization. The political parties harnessed such networks in the electoral districts to their own purposes, and the result was a pattern of politics built around local bosses or caciques also found in Latin America.[41]

By the first decades of this century, however, socioeconomic change had given rise to new, more modern parties on the center-left of the political spectrum—republicans, socialists, and anarchists in Spain; republicans and socialists in Portugal. Each was embedded in extensive, though geographically limited associational networks. Political forces on the right did not attempt to create equally modern parties to counter this challenge from the left until the 1920s in Portugal and the early 1930s in Spain, and this proved too late to match the electoral strength of their opponents, rooted as it was in a range of voluntary organizations. Hence the temptation on the part of many on the right to look to the army to help meet a threat that they believed could not be defeated by electoral means alone.[42] This temptation of course ultimately led to the overthrow of democracy in both countries.

I have argued in this essay that one will not find a parsimonious, generalizable explanation for the durability of democracy in eight Western European countries during the interwar period and for its collapse in four others in a class-coalitional analysis of the kind employed by Rueschemeyer, Stephens, and Stephens in the spirit of Moore or of the kind developed by Luebbert; nor will one find it in a historical-genealogical analysis such as that of Mann or Rokkan. A better chance of success, I have suggested, is offered by a new approach, which focuses on variations in the relationship between associational life ("civil society" narrowly conceived) and political parties across Europe during this period. Such an approach underlines the significance of voluntary associations structured along many different lines (class, religion, gen-

[41] Tavares de Almeida (fn. 30), passim; Juan Linz, "The Party System of Spain: Past and Future," in Seymour Martin Lipset and Stein Rokkan, eds., *Party Systems and Voter Alignments: Cross-National Perspectives* (New York: Free Press, 1967), 198–99, 202–8.

[42] Antonio Costa Pinto, *Salazar's Dictatorship and European Fascism* (Boulder, Colo.: Social Science Monographs, 1995), 92–106, 135–46; Linz (fn. 41), 200–201, 259–64.

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der, region, leisure-time interests) in bringing together citizens during periods of rapid social and economic change. At the same time, it highlights the ways in which political parties and associations could affect each others' development in such a way as to strengthen or undermine the prospects for democratization and democratic survival under unfavorable economic and international conditions.

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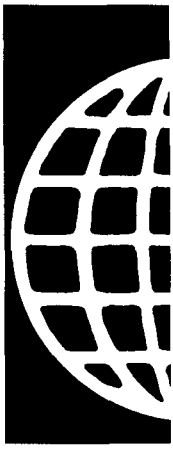

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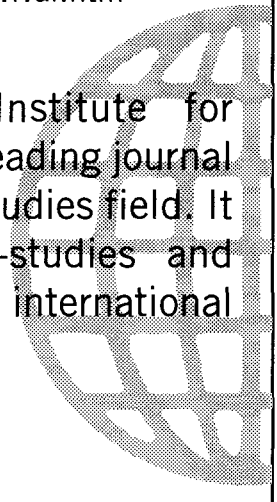

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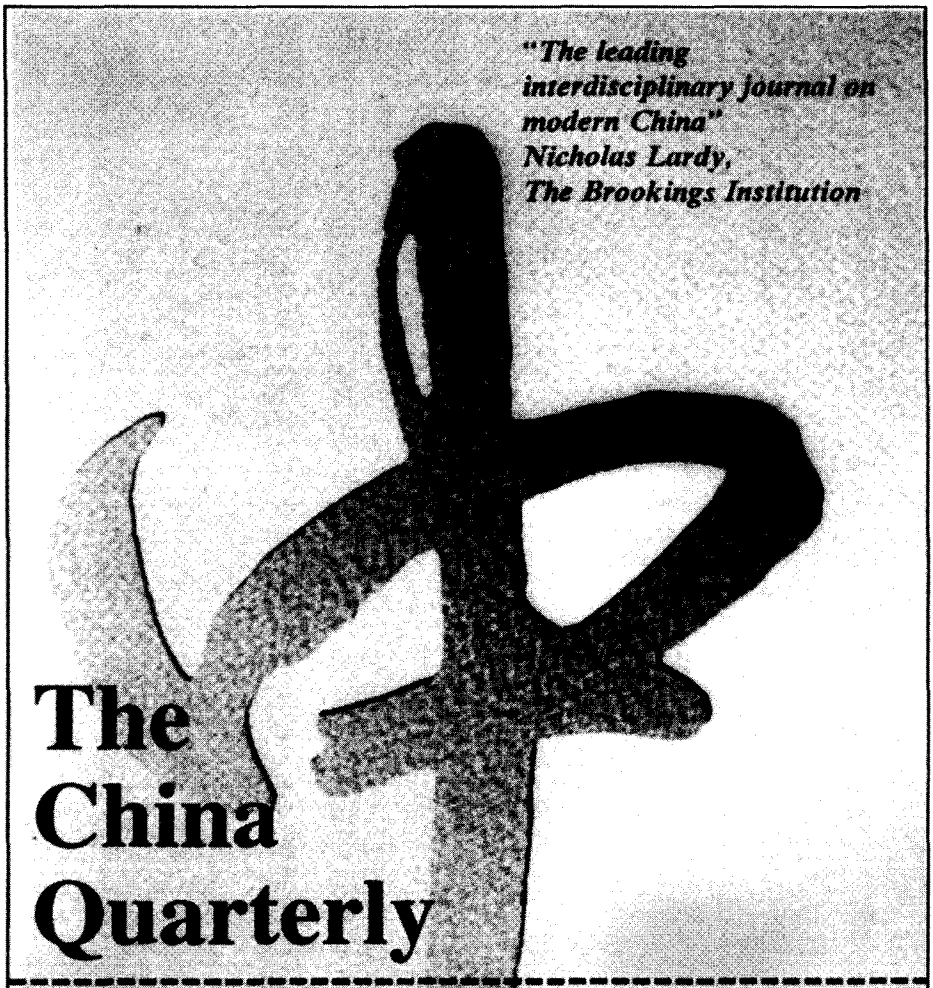

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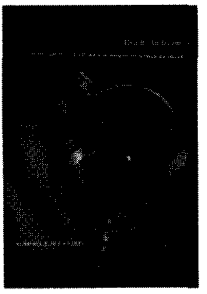

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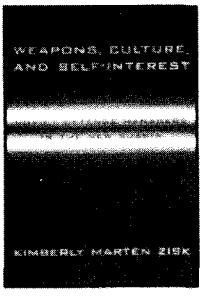

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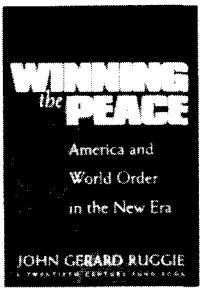

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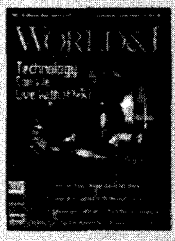

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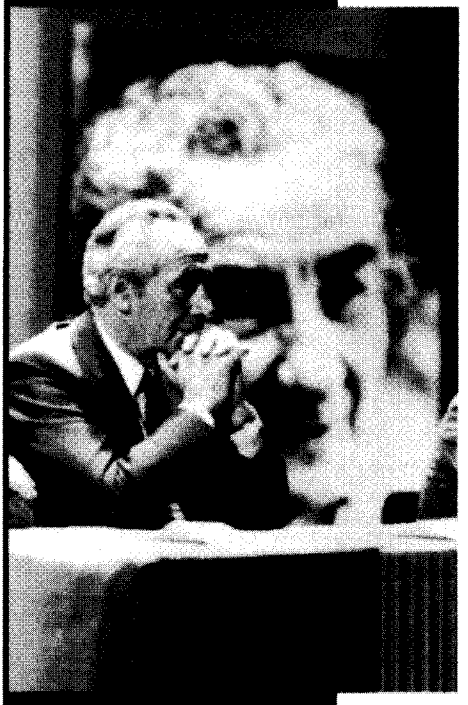

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